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NOTES ON ASIATIC MUSEUMS

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ASIA, whatever its contributions to art and science, has, humanely speaking, taught little to the west as to either the means of forming its illustrative collections or the manner of displaying them; in fact, as far as I am aware, the trend of Asiatic culture has been rather to deter its people from collecting. For such an interest, to pure eastern ideals, would foster the heresy that the things of this world are to be the more highly prized: or, in another direction, it might suggest undesirable ostentation. It is from the latter point of view, in fact, that a Japanese collector will still decline to exhibit his treasures outside of the circle of his intimate friends. In any event, whatever be the reasons, I think it may safely be said that comprehensive collections were early unknown in the east. In India, land of fabulous riches, the pre-European collections appear to have been confined to the cabinets of rulers and the wealthiest civilians, and were made up largely of decorated objects, ivories, jewels, arms, now and then menageries—the last sometimes including exotic animals. Such collections were usually little more than a gathering of valuable heirlooms, objects obtained during travels, and curiosities generally.[1] And similar conditions prevailed, as far as I was able to find, in China. In Japan, small collections were, and are, very numerous. Professor Morse, knowing his theme more accurately than Huish, describes the Japanese as a nation of collectors; but such collections, as I think all will agree, are notable for their quality rather than their comprehensiveness, and are formed in the

[1] I recall, as a typical specimen in such an early collection a copy in ivory of a human skeleton which a rajah (of Tanjore) had caused to be prepared in Paris—for a genuine one could not, according to the rules of caste, be used in his anatomical inquiries.

strictest sense for private use. In Japan, as elsewhere in Asia prior to the invasion of European methods, there was not, I believe, a single public museum, unless indeed we regard as museums the storehouses of temples. These, however, contained little more than the reserve stock from which objects for temple service or decoration were chosen.

The earliest Asiatic museum appears to have been established in the Moluccas, about half a century after their definite settlement by the Dutch, and in the classic work[2] of Georgius Everhardus Rumphius, written at the close of the seventeenth century, we have a record of the number and variety of objects which had been gathered together by this enterprising collector in the room of curiosities in Amboyna (Amboinsche Rariteitkamer). It is evident that this collection was well represented in mollusks, crustaceans and echinoderms. It contained a number of minerals and a small collection of fossils, the latter representing many groups. The descriptive catalogue of Rumphius, it may be mentioned, is well known to naturalists as containing the first account of the soft parts of the chambered nautilus, accompanied, too, by a figure which for a century and a half proved the most accurate in existence. Few details appear as to the organization of this pioneer Asiatic museum. Its founder was a well-to-do merchant in Amboyna, and it was probably installed in one of his warehouses. As far as I am aware, there is no proof that it was formally opened, in the sense of a modern museum, but by analogy of contemporary collections it is probable that the curiosity room of Amboyna was as freely open to visitors as the similar collections in London, Dresden or Paris.

In India the modern public museum found its definite foothold at the time of the extension of British rule. At the end of the eighteenth century, there were already active collectors among the officials of the East India Company, but in general the material then collected, whether ethnological, plant or animal, found its way into Europe. In the work of Linnæus, for example, we find record of many Indian species which had been sent him by European collectors. It was by such early workers in various Indian cities that societies were formed which became of considerable importance toward the middle of the nineteenth century. And it is to these local societies that the origin of many of the recent museums is due.

In the present paper it is not my plan to refer even in outline to all museums of Asia. Those of Japan are so important that they might conveniently be reserved for a separate paper. The Dutch museums, moreover, I have not had an opportunity of visiting, nor yet those on the continent in the Malayan states. When in Calcutta, I

[2] Amsterdam, François Halma, 1705. Part of the collection, as Mr. C. Davies Sherborn has kindly ascertained for me, was later sent to Europe and sold, 1682, to Cosmo de Medici III. It was subsequently transferred to Austria as part of the Medicean inheritance.

was told by Dr. Annandale of the interesting museum at Kuala Lumpur in Selangor, the federal capital of the Malayan states, which promises to be most complete. A building is here in process of construction, which will make this museum twice the size, for example, of the well-known museum at Colombo. Its present curator is the ornithologist, Mr. H. C. Robinson, formerly of Liverpool. I learned also of the museum at Thai Ping, capital of Perak, which contains a remarkable ethnological cabinet and an extensive collection of Malayan reptiles. This museum, under the direction of Mr. Leonard Wray, is, I was told, one of the most interesting in Asia. The museum at Bankok, on the other hand, is less important, in spite of the apparently more favorable conditions under which it has grown up. And its arrangement leaves much to be desired.

Of the museums in the Dutch East Indies, that at Batavia is easily the first, containing extensive local collections, both ethnological and faunistic. A second museum, at Trevandrum on the west coast of Java, has received the favorable comment of experts. Its collection of whales is especially complete.

The museums in China may be dismissed with but few words. In the Chinese treaty-ports there is little interest in museum matters on the part of resident Europeans, whose ways are commercial, and under existing conditions the Chinese authorities can hardly be expected to grant funds for such purposes. The best Chinese museum is the one at Hong Kong. It has a separate building with well-lighted galleries, and exhibits a fairly extensive series of natural history and ethnological objects, coins, etc. It is clear, however, that its resources are very restricted, and such a museum, whatever its effect upon the oriental visitor, is apt to be uninspiring. In Peking, however, in connection with the Imperial University of China, an important museum will soon be opened; it may be mentioned that this branch of the governmental educational work has been largely directed by the Japanese.

The museums of the following cities may be given a more detailed report, *viz.*, Singapore, Colombo, Madras, Calcutta, Lahore and Jaipur. The museum in Bombay is said to be uninteresting, and I neglected to visit it.

Singapore

The museum at Singapore, known as the "Raffles Museum," had its origin (1844) as a proprietary library in which local curiosities came to be preserved. In 1874 the institution was taken over by the British government (Straits Settlements), and in 1887 the present building was provided to house a collection acquired at the time of the Victorian Jubilee. The building is well proportioned, suitably lighted and planned, Fig. 1, but too small for its needs, and the authorities are now constructing an addition. This will be of the same size as the

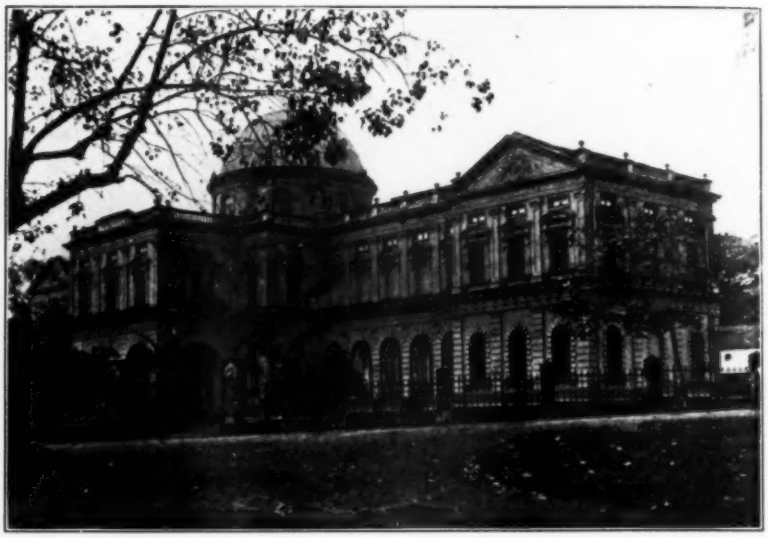

FIG. 1. SINGAPORE. THE RAFFLES MUSEUM AND LIBRARY.

earlier building, and is to be connected with it by a wide gallery passing from behind the main staircase. Each building measures about 250 feet long by 50 feet wide; the cost of both buildings amounts to about $100,000. Building, it will be seen, is distinctly less expensive than in the Occident!

The site of the museum is in a small city park. Entering the building from the town side, one passes into a spacious rotunda well filled with cases, and giving one the preliminary color of the local fauna. Prominent, for example, is a tiger fairly well mounted, and with a jungly background. This huge creature had been, I was told, the household pet of a local Rajah. One may mention, incidentally, that the tiger is decidedly on the increase in the Malay Peninsula, indeed even in the immediate neighborhood of Singapore. The collection of insects in the museum is important. In the rotunda is a series of native beetles and orthopters, including among the former, wonderful longicorns and Scarabœids; and, among the orthopters, the best examples I have seen of leaf insects and walkingsticks. At one side of the rotunda is the entrance of the Raffles Library (now grown to 30,000 volumes), which is devoted largely to works dealing with local natural history and ethnology. At the back of the rotunda, one ascends the stairs and enters the natural-history gallery and the ethnological rooms. Among noteworthy exhibits I recall the collection of local butterflies and moths, and a series, possibly the best extant, of paradise birds. The reptiles include turtles, crocodiles, and a great number of local snakes. The cases containing the gibbon and

ourang would, I am sure, be cordially envied by the best western museums, even though the mounting is not quite up to the present standard. I recall particularly one male ourang with a splendid head, and of extraordinary size. Among the zoological rarities are the relics of a very young dugong. This had been brought to the museum living and the preparations are accompanied by sketches of the living animal. In invertebrate material there is the usual range of crustacea, corals and sponges, most of them carefully determined. The ethnological cabinet (Malayan) is important, as one might expect, and its arrangement is well carried out. There are models of houses, some with inao suspended about them, suggesting primitive Japanese buildings, even with the curious "frog-thigh beams" crossing at the ridge pole, as in the most primitive Shinto temples, and with these are many suggestions of relationship with Japan. Of Dyak objects there are rich gatherings, including a collection of krisses, costumes, ornaments, etc. There are a number of the sharply-perforated carvings still used to decorate Urala ceremonial feasts, groups of objects used in marriage ceremonies, collections illustrating local basket-making, an art in which the Malayans are especially skillful. There are also cases of native cloths, coins and ornaments of gold and silver, the latter not as good in quality as one might reasonably expect. In the artistic treatment of many of these objects there are obvious affinities with the South Seas. Much of the success of the present museum has been due to the labors during the past dozen years of the director, Dr. R. Hanitsch, whose picture, as he stands in front of his bungalow, near

FIG. 2. SINGAPORE. BUNGALOW OF THE MUSEUM-DIRECTOR, DR. HANITSCH.

the museum garden, is shown in Fig. 2. Dr. Hanitsch is a graduate of the University of Jena, and was for many years demonstrator in zoology in the University College of Liverpool. The former director was the well-known ornithologist, Mr. W. Davidson.

Colombo

This museum, oldest in its building (1877) and in some regards best of Asiatic museums, was built on the outskirts of the city in the middle of the old cinnamon gardens. It is especially important to the general visitor as giving him the only practicable glimpse of the antiquities of Ceylon. It stands back from the red road, its buff-colored and long two-storied façade appearing prominently against a setting of tropical trees. On the ground floor are arranged the antiquities: in one room are objects in precious metals and stones, arm-rings, necklaces, utensils, caskets, sword handles; and near by are figures dressed in Cingalese finery of early times; on another side is a library containing Ceylonica, and a mass of the ruler-shaped books with palm-leaf pages scratched with Sanscrit; on still another side, in an imposing gallery, is a collection of architectural and decorative objects in wood and stone, including the colossal lion brought from Pollonarna, on whose back the native kings sat when they administered justice. Here also is the beautiful window from the ruin of Yapahoo, and a huge portrait statue of a twelfth-century king. On the walls of the main staircase are copies of the frescoes of the caverns of Sigiri. The collection of antiquities extends even into the garden, where several of the larger statues and a shrine are exhibited. The upper story of the museum is devoted to natural history, and here the distinguished director, Dr. Arthur Willey, has arranged groups of animals to give the visitor an adequate picture of the wild life of Ceylon. Alcoholic and dried specimens are well displayed and labeled, and even living specimens are interspersed, as in a case containing leaf-resembling insects. Dr. Willey has taken greatly to heart the need of exhibiting living creatures in the interest of his museum and, in the garden adjoining his office, he has arranged a small menagerie, which has proved a great attraction no less to foreign visitors than to natives. Nor does Dr. Willey escape his living charges even when he goes to his bungalow, for there I saw a fine series of the rare lemur, *Loris*, as well also as a specimen of *Ichthyophis glutinosa*, the earthworm-like amphibian whose development was studied by the Sarazins.

No one should leave Ceylon before paying a visit to the renowned botanical gardens, with a small museum, at Peradeniya; for it is but seventy miles from Colombo and at a delightful altitude (1,500 feet). For here within a small area one may see, with a minimum of discomfort, the rarest and most striking tropical plants, from minute orchids

to banyan trees: and one wanders about as in a land of enchantment, amid traveler's palms, which will spout water if one punctures a stalk, breadfruit, cocoanuts, nutmegs, cinnamon, deadly upas trees, *Bauhinia racemosa,* with its cable-like stems, and the telegraph-plant, *Desmodium gyrans,* automatically lifting and dropping its leaves. Incidentally, too, there are zoological interests. Not uncommon are trees infested with flying foxes: and in the neighborhood the traveler to the east may see his first elephant working in the fields, but willing to show his paces for a few pice; so too one might happen to make the acquaintance of land-leeches, which find their way unpleasantly through the

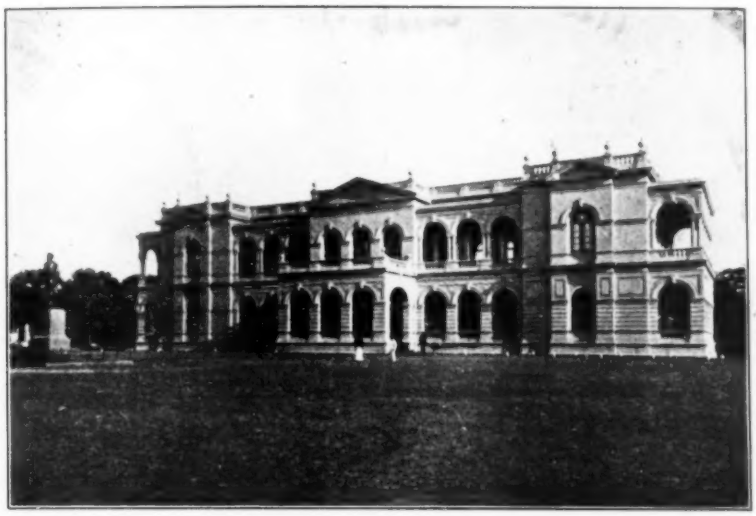

FIG. 3. COLOMBO. CITY MUSEUM.

buttonholes of his shoes. But as an offset to this he may see a wild peacock, glorious in color. Or he may discover a cobra and induce it to display its hood.

MADRAS

The museum in Madras is in many regards a quite modern institution. Its buildings are new and spacious, built of dark brick and terra-cotta in Indo-Saracenic style, Fig. 4. Its collections illustrate admirably the natural history, archeology and art of southern India. Included with it, also, is the important Connemara library, rich in material relating to the history of Madras. The natural-history section is the oldest of the museum, part of its collection dating from 1846, Fig. 5, and it has the interest of including within its animal galleries a number of living specimens. The archeological section is rich in prehistoric objects, especially pottery: it contains, however, many objects of the sixteenth and seventeenth centuries, arms, armor and cannon, of the days of European as well as of native wars. Among other curious

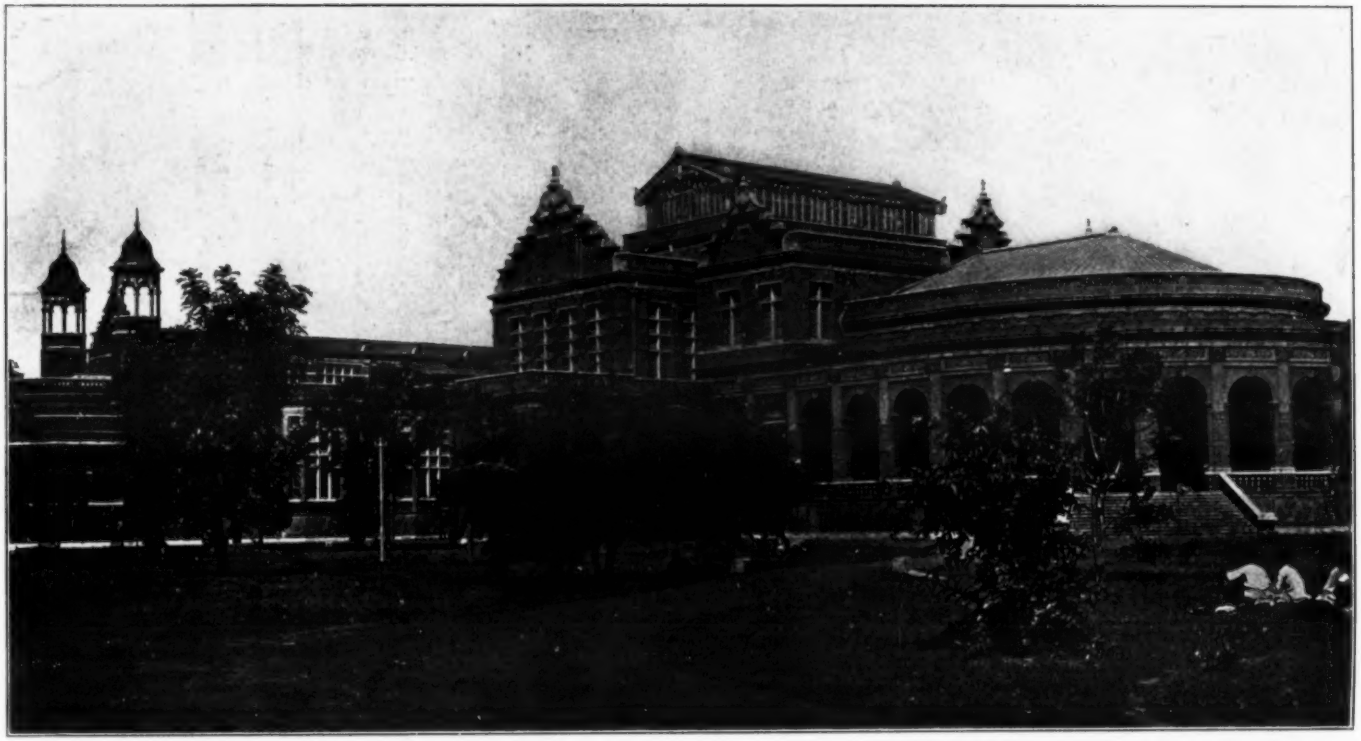

FIG. 4. MADRAS. MUSEUM BUILDING.

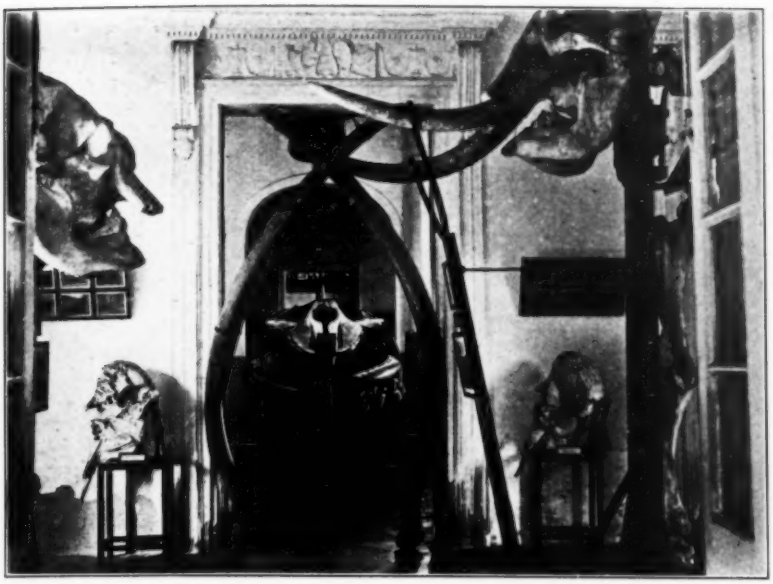

FIG. 5. MADRAS. VIEW IN ONE OF THE NATURAL HISTORY GALLERIES.

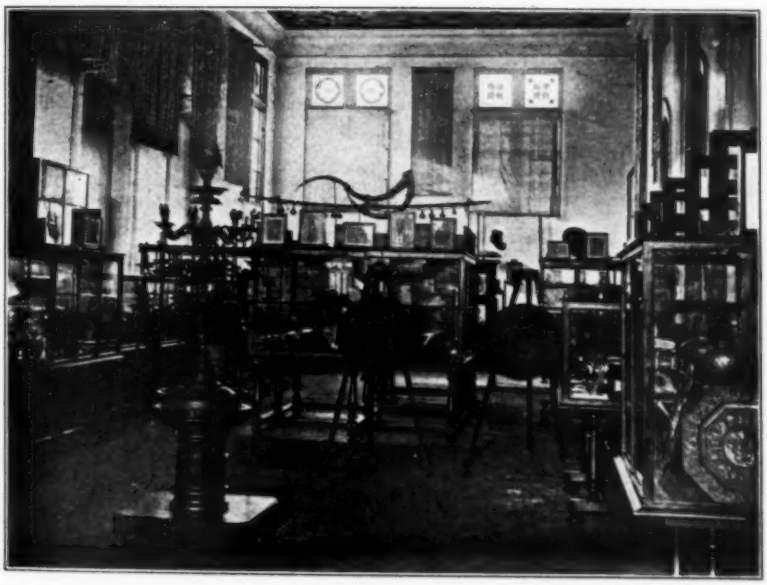

FIG. 6. MADRAS. GALLERY OF METAL WORK.

relics is a large swinging-post terminating in an elephant head, probably unique, which in a remote village was used up to relatively recent times for human sacrifices. The art objects are represented in great variety and are attractively exhibited, textiles, pottery, wood and metal work, musical instruments, drawings. One recalls especially the suite of pictured cotton curtains for which Madras has long been noted; also the beautiful repoussé work in precious metals (Fig. 6). The museum is distinctly one of the most successful in India. Its director is the zoologist, Dr. Edgar Thurston.

Calcutta

The museum of Calcutta is far and away the most imposing of Asiatic museums, representing, as it does, the government of India in the imperial capital. Its buildings, Fig. 7, are the most extensive and its collections the most important. In this region, moreover, it is the oldest, for it preserves the collections of the Asiatic Society of Bengal, founded in 1784.

The success of the museum, it may be remarked, has been due in no small degree to its tradition of selecting directors eminent both as scientists and as executives. It was to Mr. Bly, an early curator of the Asiatic society (1842 to 1862), a voluminous correspondent of Darwin, by the way, that the credit belongs for securing governmental assistance in erecting the museum's first building. His successor was John Anderson, who remained in charge until 1886. And his, in turn, was Dr. Wood Mason, 1886 to 1893. And from that time to the present, the director has been Major A. Alcock, widely known for his researches on the deep-sea fauna of the Bay of Bengal.

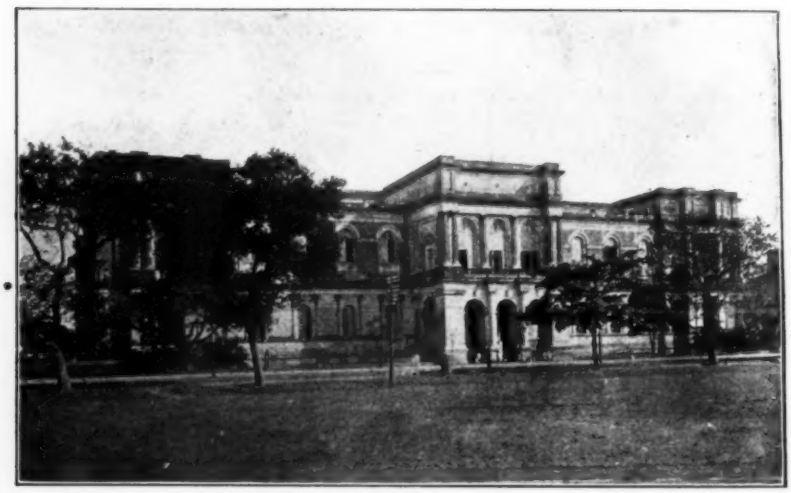

Fig. 7. Calcutta. The Indian Museum. Front view. From Chowringhee.

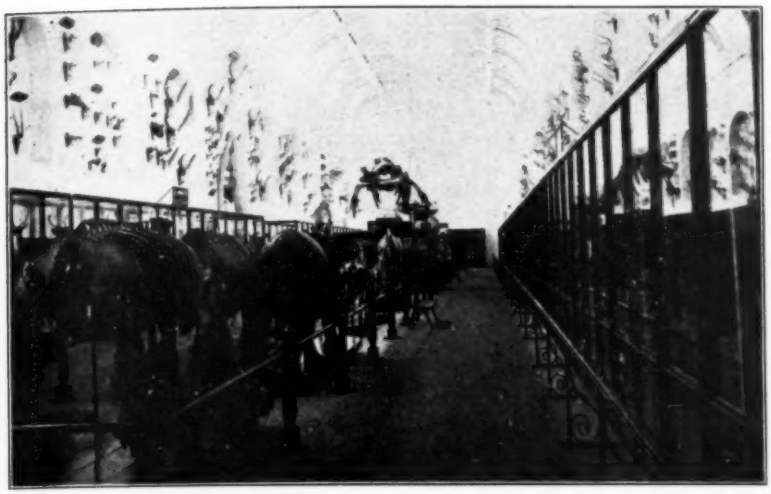

FIG. 8. CALCUTTA. HALL OF INDIAN MAMMALS.

At the time of the opening of the new museum (about 1890) the collections of the Asiatic Society were transferred to the British government. They comprised principally three classes of objects, zoological, ethnological and archeological, the last of unique importance. They include the antiquities secured by Colonel Mackenzie from the Amravati tope (1796 and 1816), and the collections of the Tytlers, Kittoe and General Cunningham. The last named investigator, one of the founders of the museum, secured for it also the objects from the Bharhut stupa. The entire collection thus contains in large measure the figured specimens in Indian archeology and it is especially rich in the finds from the neighborhood of Lucknow, Nagpore, Benares and Delhi. The ethnological cabinet is based upon the collection of Roer, whose catalogue dates from 1843. By 1882 no less than 600 crania were listed. The zoological division of the museum is based upon the Blyth collection of the Asiatic Society. As early as 1862 there were represented 600 species of mammals, 2,000 species of birds, 300 of reptiles, and 1,000 of mollusks; and since this time the zoological collection has increased vastly. Figs. 8, 9, 10.

The Calcutta museum expanded notably about two decades ago, when it incorporated two allied institutions. The first of these was the economic museum of the government of Bengal (added in 1887), whose collections are arranged in separate galleries, and the second, the collections of the geological survey, these added (about 1890) when the public museum was opened. The subsidy for the latter institutions, it may be mentioned, is separate from that of the main museum, about 40,000 rupees a year being granted by the government for their annual support. And a similar appropriation is made for the remainder of the museum.

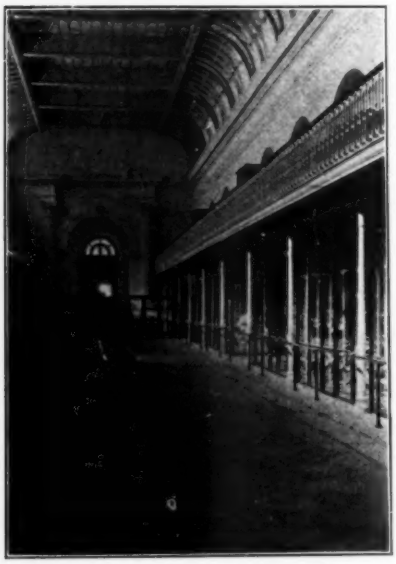

FIG. 9. CALCUTTA. PORTION OF THE BIRD HALL.

Under the present director the work of the museum has made notable advances. During the past twelve years over 100,000 specimens have been entered in the books of the museum and the new material has been extensively studied. Especially through the cruises of the *Investigator* carried out under Major Alcock's direction (Major Alcock came to India as surgeon-naturalist (1888–1892) to examine the sea-barriers of India), a wealth of marine material has been placed in the hands of specialists throughout the world. And the museum had already published many memoirs upon it—twenty-five, or thereabouts. It might be mentioned, as a sad commentary upon the relation of politics and science in India, that the well-known gallery of fishes arranged by the director, after years of labor, has recently been demolished by order of the Viceroy, Lord Curzon, who could find in Calcutta no other gallery in which to house a collection of relics of the Sepoy rebellion!

The invertebrate collections of the museum are extensive and well displayed. Particularly interesting is the entomological cabinet which includes the de Nicéville lepidoptera and the Dugeon hymenoptera, the latter comprising about 1,000 type specimens. The entomological survey undertaken by the museum is its last development, establishing in 1903 the first entomological laboratory in India, in connection with a commission of forestry. Equally important are the geological materials exhibited in the museum. Of meteors, no less than 400 falls are represented. Of ores there are many varieties, especially in manganese. In fossils there is valuable Cretaceous material, including the types of Blanford; among late acquisitions there is a wonderful specimen of *Elephas antiquus* (*namadicus*). The fossil mammals from the Sewalik Hills near Simla are also preserved in the

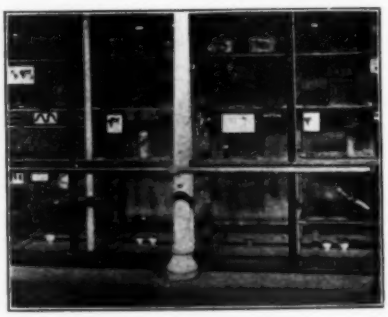

FIG. 10. CALCUTTA. A CASE IN THE REPTILE GALLERY.

gallery of paleontology, but they fail to impress a visitor who has seen the associated remains of late Tertiary mammals in other museums.

Lahore

The museum at Lahore is known to most foreigners as the "wonder-house" of Kipling, and in front of its door stands the ancient cannon with its memories of Kim and his lama. Although intended to represent the natural sciences as well as the arts, this museum need hardly be referred to in the former regard, for its specimens are few and poorly displayed. In its materials for the study of art, however, it ranks among the foremost in the east. Its predecessor was a school of arts, founded as a memorial to the Viceroy, Lord Mayo, and carried out during the early seventies, under its first principal and curator, Mr. J. Lockwood Kipling (1875–92). The development of the present museum then came about as a result of the Victorian jubilee. A general subscription secured the necessary funds, and the corner-stone of the present building (Fig. 11) was laid by Prince Victor in February, 1890, and its collections were opened to the public two years later. The design was furnished by Mr. Lockwood Kipling in cooperation with the Indian architect Bryam Singh.

As in the majority of the Indian museums, the native style has been as closely followed as museum needs would permit, and the tall galleries and massive doorways (Fig. 14) leave pleasant impressions in the

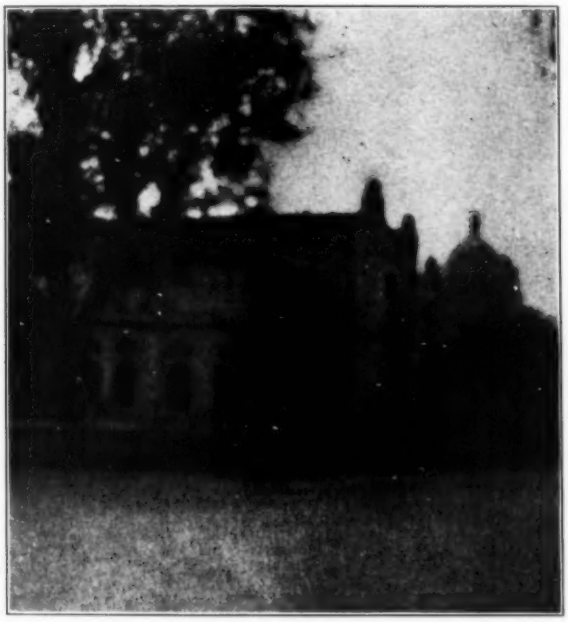

Fig. 11. Lahore. The Museum.

FIG. 12. LAHORE. THE HALL OF GRÆCO-BACTRIAN SCULPTURE.

visitor's mind. The exhibit space includes about 28,000 square feet and the galleries are 45 feet high. As already noted, the museum is interesting in its art exhibits, especially in its Græco-Bactrian sculptures, for these, as is well known, played a most important part in the early art of northern India. This collection, occupying a special gallery 100 feet in length (Figs. 12 and 13), was brought together in the northwest provinces during the early seventies, and is unique. To be mentioned also are the collections of carved wood, musical instruments, Hindu portraits, including a series of the Singh, Hindu drawings, many Afghan documents, and technical exhibits decidedly modern in museum technique, illustrating, for example, the arts of the Punjaub, glass making, lac turning, leather work, etc. In connection with these there are models of local industries cleverly carried out in terra-cotta by native artists. One may mention also a remarkable series of Madras curtains elaborately stamped with religious ceremonies and personages. The present administration of the art school and museum is in the hands of Mr. Percy Brown, artist and archeologist, well known for his studies on Græco-Bactrian art. The museum is now affiliated with the Asiatic Society of Bengal, with the Geological Survey of India and with the Forestry Commission. As an echo of Indian social conditions one hears that the museum has been opened one day a month for Hindu women, women attendants then taking charge of the galleries. The museum is popular, and the attendance averages over 1,000 a day.

FIG. 13. LAHORE. DETAIL IN HALL OF SCULPTURE.

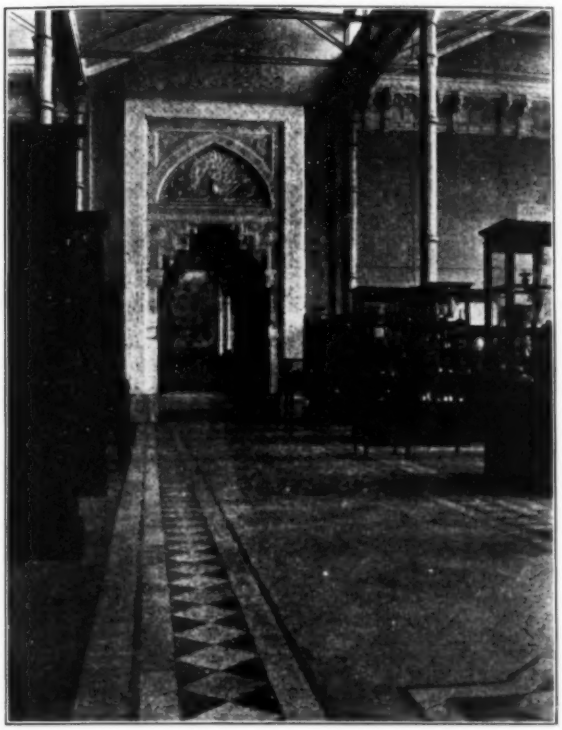

FIG. 14. LAHORE. THE HALL OF NATIVE ARTS.

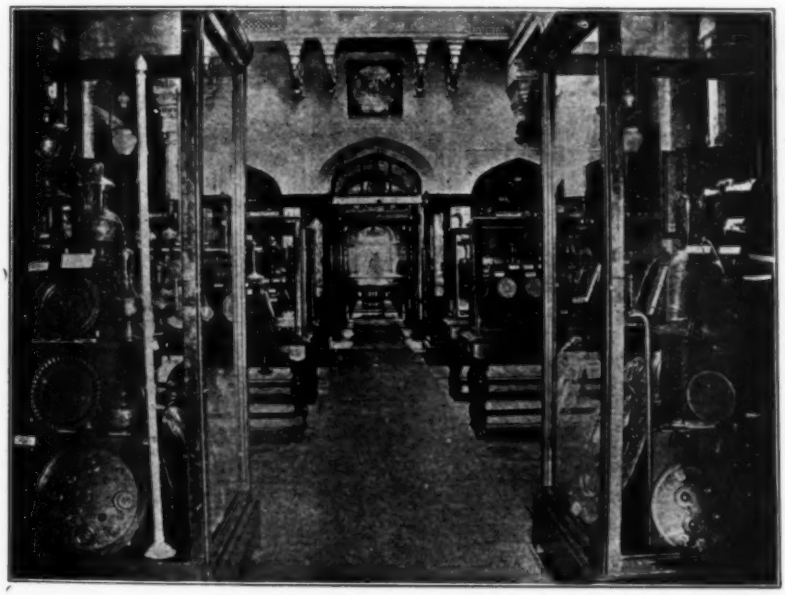

FIG. 15 JAIPUR. HALL OF METAL WORK.

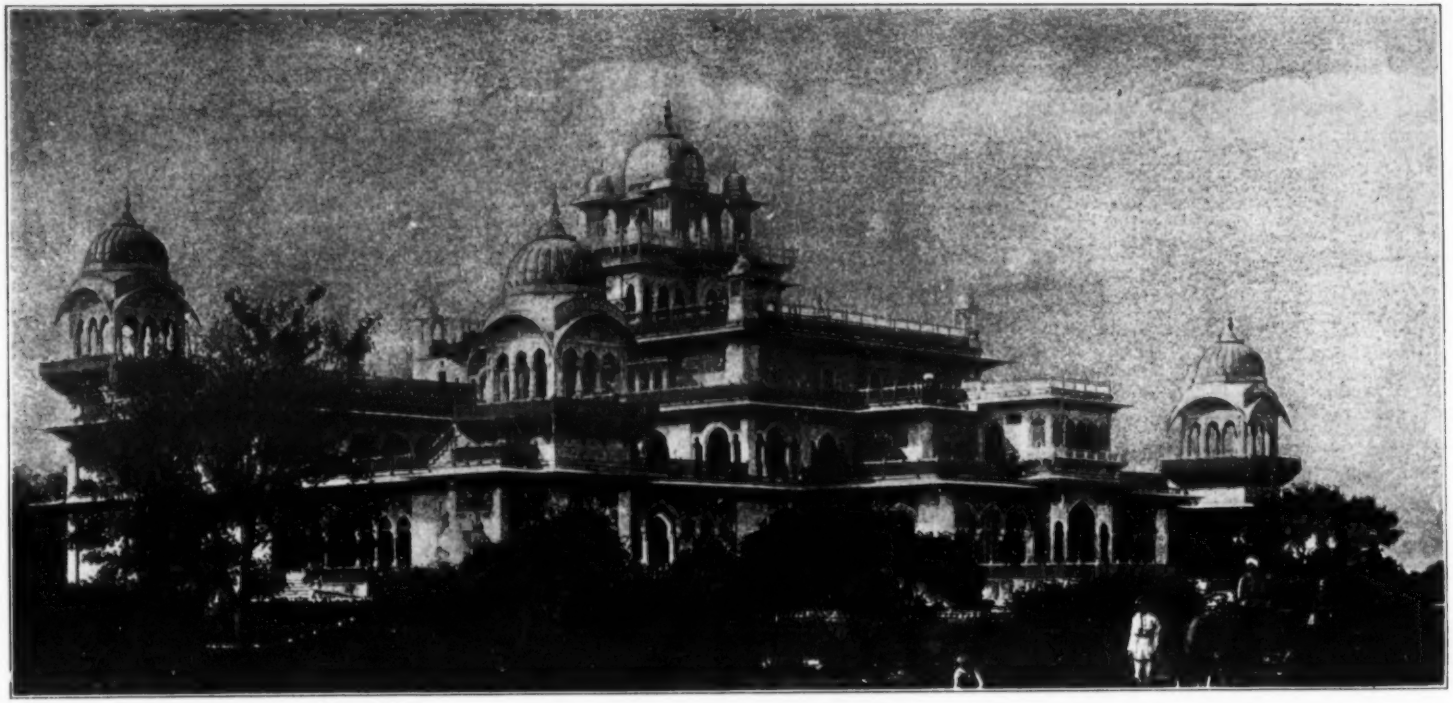

FIG. 16. JAIPUR. THE MUSEUM.

Jaipur

Jaipur may be mentioned, finally, as furnishing the best type of a museum supported by a native prince—in the present case by the reigning maharajah, Sir Sawdi Madho Singh. It is an imposing monument to this ruler's modernness, and it has already borne interesting fruit in developing and bettering the many art-industries of Jaipur.

The building is by no means a small one—at least two hundred feet in length. It stands in the public gardens, an elaborate structure in Indo-Saracenic style, with shaded balconies and corridors, and with numerous courtyards cooled by plants and fountains, Fig. 16. Its scientific collection is small, limited to models and specimens of minor interest. But in modern and semi-modern art objects, in metal, stone, wood or textile, the present museum is, I believe, unsurpassed. Especially beautiful are the examples of metal work, Fig. 15, many of which are the family treasures of the maharajah—gun-metal and silver bidri work, damaskeens from Kashmir, silver repoussé from Trichinopoly and Ceylon, articulated objects in silver from Bengal, silver figures from Mathura, enamels in gold from Jaipur, in silver from Multan, brasses numberless, and a bewildering series of jewelry from all parts of India. Nowhere can one receive a more illuminating impression of the decorative possibilities in native art. An excellent reference, by the way, is the beautifully illustrated handbook of the museum prepared by its honorary secretary, Colonel Hendley (1895).

THE PLACE OF LINNÆUS IN THE HISTORY OF SCIENCE[1]

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THE recent celebrations of the bicentenary of Linnæus's birth had one sort of appropriateness in somewhat higher degree than is usual in such commemorations: they helped pay the debt of posterity to one of the great figures of the history of science in the currency that he had especially valued. For Linnæus had very markedly the last infirmity of noble mind. *Famam extendere factis* was his chosen device, which he often prints, with a pride justified only by the event, upon the title-pages of his books; and his biographers are at one in emphasizing the intensity of his desire for fame. It was, indeed, the solid and enduring fame of the productive scholar that he sought, not the applause of the groundlings; his ambition was to link his name to some lasting and imposing part of the ever-enlarging fabric of organized knowledge, and thereby to take rank among the acknowledged masters of those who know. That this ambition, large as it was, has been more than fulfilled, is sufficiently evidenced by the world-wide commemoration of this anniversary of his birth—even in cities of the western continent which were themselves non-existent when he came into the world. No naturalist of his century, and few naturalists of any period, have so universal a popular reputation, or are, by so nearly common consent, given a place among the immortals not far removed from Copernicus, Galileo, Descartes, Leibniz and Newton—to mention only his predecessors. Yet, when seriously scrutinized, Linnæus's position in the history of science is a peculiar one. With his name there is commonly associated no epoch-making hypothesis, not a single important discovery, not one fundamental law or generalization, in any branch of science. The forty years of his active life constitute a period prolific in fruitful hypotheses and signalized by the original enunciation of a number of valid generalizations of the first order of importance; of none of these was he the author. To go no farther than the biological sciences which Linnæus professed: Before 1750, Daubenton and Buffon had begun to establish the new science of comparative anatomy and were making known the striking homologies which run through the structure of all species of vertebrates; between

[1] Revision of a paper read before the Academy of Science of St. Louis at its celebration of the two hundredth anniversary of the birth of Linnæus.

1745 and 1751 Maupertuis had promulgated, and defended with effective arguments, the theory of the transformation of species; in physiology, the significant fact of the independent irritability of muscle was discovered by Haller in 1757; in embryology, the doctrine of epigenesis was revived and finally established by Caspar Friedrich Wolff in 1759. As for the science of botany, the foundations had been laid, and the general outlines and principles which were to continue to rule during Linnæus's time had been established by the end of the preceding century. The founder of modern scientific botany is Cesalpino (1583). In microscopic plant anatomy and histology, the investigations and descriptions which were to underlie the science for something like a century had been made before Linnæus's birth by Grew, Malpighi, Leeuwenhoek. In plant physiology, the rôle of the sap had been studied by Malpighi, and the fundamental facts made clear by Hales in his "Vegetable Staticks," 1727; the function of pollen in the fecundation of seeds had been shown by Camerarius before the end of the seventeenth century; the existence of the sexual distinction in plants had been insisted upon by a long succession of botanists, English, German, Italian and French; and during Linnæus's lifetime the physiological rôle of leaves was being made clear (so far as the condition of chemistry at the time permitted) by the philosopher Christian Wolff[2] and by Bonnet.[3]

Not only is all this true, but it is also a fact that Linnæus has been not absolutely unfairly represented, by one of the historians of modern science, as an obstacle to the scientific progress of his time. President White, in his "Warfare of Science and Theology," after speaking of certain anticipations of nineteenth century conceptions by DeMaillet, Robinet and Bonnet, remarks:

> In the second half of the eighteenth century a great barrier was thrown across this current—the authority of Linnæus. . . . The atmosphere in which he lived and moved and had his being was saturated with biblical theology, and this permeated all his thinking.

Yet, though in the intellectual movement of his time Linnæus was an extreme conservative, if not something of an obscurantist; though he was far surpassed by several of his contemporaries in that kind of insight and constructive power which leads to the discovery of the great general laws of nature; and though the heavy pioneer work even in his favorite science had been done before his time by the great investigators of the end of the seventeenth century—though all this is the case, none of these others equals Linnæus in popular repute or in accepted standing in the history of science. I can not say that I think this altogether just, though if it be less than just, the proper inference

[2] "Entdeckung der wahren Ursache von der Vermehrung des Getreydes," 1718. Cf. also his "Vernünftige Gedanken von dem Gebrauche der Theile in Menschen, Thieren und Pflanzen," 1725, Pt. II., chap. 5.

[3] "Récherches sur l'usage des feuilles," 1754.

is not that we should praise Linnæus less, but some of the others more. I have, however, mentioned these things, not for the sake of measuring out Linnæus's glory with a hopeless attempt at exact distributive justice, but for the sake of defining more precisely, and in terms of explicit contrast—which is the only illuminating way of defining—the nature and limits of Linnæus's contribution to the evolution of the sciences. He was the one naturalist of first eminence whose work lay entirely, or almost entirely, within the sphere of descriptive and classificatory science. His rôle is precisely described by the term which he himself employed; he was not the originator of, nor a great discoverer in, botany, but he was the "reformer" of that science, *reformator botanices,* and in a less degree, of zoology. And in using this term to describe his work, the emphasis should be upon the "form." He was, in other words, an unsurpassed organizer, both of scientific material and of scientific research; he introduced form and order, clearness and precision, simple definitions and plain delimitations of boundaries, into sciences previously more or less chaotic or confused or impeded with cumbrous and inappropriate categories and terminology.

This reformation was the result of the three improvements effectually introduced by Linnæus and indissolubly associated with his name. The first, which seemed the most impressive and did most to establish his fame among his contemporaries and for several generations thereafter, was really the least permanent and the least valuable of his contributions: this was the introduction of a new artificial system of classification, based, in the botanical field, upon the differences of the sexual organs of plants. The second was the introduction of the binomial nomenclature, the system of so-called "trivial" names, which put a final end to the hopeless length and complexity of botanical and zoological specific names, and sharply differentiated the naming of organisms from the description of them. The third and, I suppose, the most useful as well as most durable of all of Linnæus's improvements, was the establishment of a new descriptive terminology in botany, the drawing up of a set of terms, each with clearly defined meaning, for designating concisely the distinguishable parts and organs of plants, and the several types of form of which each part is susceptible. By these means Linnæus imposed order and harmony upon a realm that had hitherto suffered much from anarchy; he gave a common language to those who tilled its fields, and provided them with working tools of an unprecedented simplicity and convenience. And where he thus introduced order he also, as a natural consequence, introduced abundance. Both directly and indirectly Linnæus immensely augmented the store of concrete botanical information. The science thus simplified and systematized and given a convenient means of expression became vastly more attractive and interesting; in particular,

it came to be a field in which many minds, of all orders of ability, could do useful work, and could make their work dovetail into the work of others in such wise that each was conscious of having contributed a definite part to an immense and impressive edifice of an intelligible outline and design. Alike by the superior convenience of his classification, nomenclature and terminology, by the force and serious enthusiasm of his personality, and by the example of his admirably exact observation, Linnæus stimulated a prodigious amount of ardent and careful botanical and zoological research on the part of others. His own pupils went out, literally by the score, not only over Europe, but to the uttermost parts of the earth, to collect new species and study geographical distribution. A number of these young enthusiasts, whose names are honorably recorded by one of Linnæus's biographers, lost their lives in these expeditions. The eight volumes of Linnæus's "Amœnitates Academicæ" contain 186 dissertations by almost as many of his pupils, the subject and treatment being in nearly every case suggested, and the results corrected, by Linnæus himself; most of these contain contributions of valuable—and many contain what were in their day highly original—botanical, zoological or mineralogical data. Nor was the effect of Linnæus's simplification and systematization of botany limited to the setting of other and younger men of science to work. His efforts also notably increased the general vogue of botany, as a result of which it long enjoyed an exceptional popularity and an unusual amplitude of endowment among the sciences.

This aspect of Linnæus's work is effectively presented—all the more effectively for a considerable touch of rhetorical exaggeration—by Magdeleine de Saint-Agy in his continuation of Cuvier's "Histoire des Sciences Naturelles" (1845); the passage illustrates so well, if not precisely, what Linnæus did, at least what he had the credit of doing, that I venture to translate it.

The influence of Linnæus, says this historian, was not limited to the investigations and voyages which he caused to be made; in imitation of him, similar voyages and investigations were ordered made by several states. Sweden, being a small and poor country, had no great means for multiplying such expeditions; but England, France and Russia had them carried out in great numbers; and Linnæus during the last years of his life had the pleasure, as Condorcet puts it, of seeing nature interrogated on all sides in his name. There was no class of people—even to princes—who did not busy themselves with natural history, and above all with botany—since this science presents none of the difficulties of anatomy and since the method of Linnæus is of a simplicity which renders it accessible to everybody . . . Botany thus became universally familiar. Those who were fond of gardening multiplied the varieties of their plants, since they could now know the names of them without being Latin scholars, and since gardeners could now understand one another when referring to the plants they cultivated. All gardens, both botanical and pleasure gardens, were filled with a multitude of plants which rich folk had brought at great expense from foreign

lands. The taste for botany dominated all minds; kings became botanists, properly so called, and were desirous of having their own botanical gardens. Louis XV. had the garden of Trianon; George III., that of Kew; Francis I., emperor of Austria, that of Schoenbrunn. These three princes were useful to the science by their gardens and by the emulation which they occasioned; but it is after all, to the happy discovery of a dual nomenclature that these advances were primarily due. From the moment when common names were to be had, corresponding in all parts of the globe, collections were zealously made; museums were enriched; and it was not difficult to multiply researches, now that the science was within everybody's reach. . . . Such is the prodigious impulsion that Linnæus gave to the science of natural history.

Yet it is important, in the interest of historical truth, to point out that even in these things which constitute his peculiar work—specifically, in his reformation in taxonomy, nomenclature and terminology—Linnæus was in no respect a pioneer or an originator. It was his good fortune to be able to develop and carry through suggestions and outlines of procedure which had been made by his seventeenth-century precursors, and to exploit to the utmost an abundant legacy of botanical knowledge, methodological ideas, and botanical interest which had come down to his generation. Nothing, indeed, could be farther from the truth than the notion which appears to have wide popular currency, that there was little botanical study or knowledge worth mentioning before Linnæus. It is, on the contrary, eminently a case where *vixerunt fortes ante Agamemnona.* Any who suppose sixteenth and seventeenth century botany to be a negligible quantity will find it instructive to examine the shelves of the library of the Paris Jardin des Plantes; or to remember that Jean Bauhin's "Historia universalis plantarum" (1660), consisting of forty books, contained descriptions of some 5,000 plants, with 3,500 figures, and cost the equivalent of about $18,000 to produce—or that, a little later, Ray's "Historia plantarum generalis" gave a classified arrangement and description of 11,700 plants.[4] And while Linnæus assuredly gave, as has been said, a great impulsion to the popular and fashionable interest in botany and zoology, it was an interest which was extremely well developed before his time—which, in fact, made his own work and his own contemporary fame possible. It was not through his influence first that states and monarchs learned the propriety of establishing botanical gardens. The Jardin royal du Louvre, for example, was established by Henri IV. in 1590, and the Jardin des Plantes was founded in 1626. By the middle of the seventeenth century both public and private gardens, often with scientific establishments connected with them, were becoming fairly common. And, as I have said, the particular reforms through which chiefly Linnæus achieved his results were essentially not discoveries nor innovations of his own. It will be profitable to note

[4] These figures are taken from Hoefer's "Histoire de la Botanique," 1872.

briefly the earlier history of the ideas involved in each of these three reforms.

First, then, concerning classification. Linnæus's great precursors in this field were Cesalpino,[5] Ray and Tournefort. Cesalpino was a sixteenth-century enthusiast of the revival of the Peripatetic philosophy; and it was largely the influence of a fresh study of Aristotle's logic and metaphysics which led him to condemn all the then customary ways of classifying and naming plants—by their medicinal or other practical properties, the localities in which they are found, and the like—as being based upon mere "*accidentia*," and to insist upon the necessity of an orderly arrangement by genera and species founded upon the presence of common visible characters.[6] In his selection of the characters by reference to which the primary division into genera is to be made, he is guided by considerations drawn from the Peripatetic metaphysics. The essential character of any "substance" consists in its "end" or "function" (*opus*). The distinctive function of the vegetative soul is twofold, nutrition and "the generating of its own like"; the latter is the higher, and it also presents more numerous and sensible points of variation in different plants. It follows that plants should be divided into genera according to the differences in form and arrangement of their "fruit-producing" organs (*ex modo fructificandi, ex propriis quae fructificationis gratia data sunt*). With this as a starting-point, Cesalpino proceeds to a series of successive divisions in which 840 species find place. Ray's contributions to taxonomy had less success and influence than those of Cesalpino and of Tournefort, and are therefore historically less significant; but concerning their intrinsic merit it is worth while quoting the recently expressed opinion of a living botanist of high authority, who places Ray[7] as a taxonomist above Linnæus himself. It was the English naturalist, says M. Bonnier,[8] who must be regarded as "the true founder of the natural method"; "he it was who first enunciated the essential principles on which the classification of plants ought to be founded, who made clear the difference between phanerogams and cryptogams, who discovered the distinction between monocotyledons and dicotyledons, who established in a rational manner the main divisions of the vegetable kingdom."

[5] 1519–1603. Cesalpino was a physician to Pope Clement VIII., and professor of *materia medica* and director of the botanical garden at Pisa. He was the original discoverer of the circulation of the blood; the doubts which have been sometimes expressed whether he anticipated Harvey's conception in its fullness have been shown to involve the overlooking of an explicit passage in Cesalpino's "De Plantis" (1583): cf. Du Petit-Thouars in "Biographie Universelle," *s. v.*

[6] "De Plantis" (1583), *Lib.* I., Cap. XIII.

[7] "Historia Plantarum," 1686.

[8] "Le monde végétal," 1907, pp. 48–9.

At a natural method Tournefort made no more attempt than did Linnæus. But of the principles and purposes of a good artificial classification he had an entirely clear comprehension; and of such a classification of then known plants he gave an elaborate and imposing exemplification. Of what a "natural system" would be, if one could attain to it, Tournefort, like his Swedish successor, had a conception rather mystical or theological than scientific; it would be an arrangement of animals and plants according to the "natural" or "essential" species established by "the Author of Nature." But for his actual scheme[9] he recognizes plainly that the primary criteria are the practical ones of simplicity and convenience. A genus or species, for botanical purposes, is "simply the whole group of plants that have a character in common which essentially distinguishes them from all others"; and in the selection of the characters by means of which the division is to be made we may ignore metaphysical considerations. Tournefort observes (apparently reflecting upon Cesalpino): "Let no one say that, since the sole end of nature is the production of fruit, we ought to consider the fruit as the noblest part of the plant. The intentions of nature are not in question here, nor yet the nobility of the several parts; what concerns us is to find means of distinguishing different kinds of plants with the greatest possible clearness. If the least of their parts served this purpose better than those which are called the noblest, it would be necessary to prefer the former." Tournefort's actual classification, based upon the characters of both flowers and fruit, realized these ideals of serviceableness, convenience and consistency somewhat imperfectly. But it was the ruling one in the science for nearly half a century; and, accompanied as it was by careful descriptions of an immense number of species, it furnished a model upon which Linnæus needed only to improve.

The Swedish naturalist's simplification of nomenclature was not only approximated, but acually anticipated, by at least one of his predecessors. As Professor Underwood has pointed out, the binomial system of naming plants was used by Cornut in his "Canadensis Plantarum Historia" as early as 1635.[10] Later Tournefort, a botanist of greater eminence and influence, though he followed this example only partially, insisted emphatically upon the need for a reform and simplification of nomenclature. So far as the names of genera are concerned, he observes that "one ought to make a very great difference between naming plants and describing them"; he remarks that "nothing is so unfavorable to the reformation of botany as the habit which

[9] "Elemens de la Botanique," 1694; the Latin version of this, "Institutiones Rei Herbariæ," with some alterations, appeared in 1700.

[10] Underwood in *Torreya*, October, 1903, and in POPULAR SCIENCE MONTHLY, June, 1907. A brief and often binomial nomenclature is ascribed by Bonnier to Belon (*d.* 1574), whose work I have not seen.

has come to prevail of judging of the nature of plants from the etymology of their names," and recommends that generic names be formed exclusively "out of words that have of themselves no meaning"; and he ridicules the long descriptive names then used by many botanists.[11] The designations of species, however, he considers, should consist of the name of the genus *plus* a clear descriptive indication of the differentia of the species; and since the latter can not always be expressed by a single word, Tournefort does not employ a uniformly binomial nomenclature. But from the reforms already recommended and adopted by the great botanist of the preceding generation to the Linnæan system of "trivial" specific names, the step was easy and obvious.

Again, in providing botany with an appropriate set of terms for the concise indication of the parts and organs of plants, Linnæus was merely following the suggestion and extending the work of another great seventeenth-century reformer in science. It was Joachim Jung[12] —a naturalist whose intellectual force so impressed his contemporaries that Leibniz did not hesitate to compare him to Aristotle, or Comenius to liken him to Euclid—who was the father of comparative morphology in botany, who introduced into the study of the characters of plants real thoroughness and precision, who insisted upon the need for a system of clear, unambiguous organographic terms, and who himself devised and introduced a number of the terms still in use. His "Isagoge Phytoscopica" (1622) was wholly devoted to urging and exemplifying this reform; all the principal parts of plants are distinguished and defined with admirable clearness, their possible variations of form noted, and new and explicit names for these variations proposed. Jung seems,[13] for example, to have been the first to employ the terms petiole or pedicule and perianth; to classify the arrangements of leaflets as digitate and pinnate, and to subdivide the latter sort into paripinnate and imparipinnate; to speak of the disposition of leaves as opposed, alternate, triangulate, etc. The descriptive terminology of botany has, of course, since expanded immensely; but the credit for the origination of the language of that science must unquestionably be assigned to Jung and not to Linnæus.

It still remains true, however, that Linnæus united these three reforms in a single system; that he carried each of them farther than had any of his predecessors; and that by the force of his personality he was able to gain for them a general acceptance which they had hitherto lacked. Though we must, therefore, make some deduction

[11] "Elemens de Botanique," 1694, pp. 14, 36, 38.

[12] Born in Lübeck, 1587, died at Hamburg, 1657. He published comparatively little, and his principal botanical works were brought out by friends after his death.

[13] The assertion that Jung was not anticipated in the use of these terms rests upon the authority of Hoefer, "Hist. de la Botanique."

from the current view of the originality of Linnæus's work as reformer and organizer of botanical knowledge, we need not on that account greatly lower our estimate of its actual importance in the history of science. And yet we must, to get a just picture, always remember the character, as well as the magnitude, of that work; we must remember that it was, all but exclusively, form, system, nomenclature and specific observations that Linnæus contributed to the biological sciences, rather than fundamental discoveries, pregnant hypotheses or illuminating general ideas. Even in the presence of the impressive picture of the solid results of Linnæus's life-work drawn by the French historian of these sciences, one can not help recalling a caustic remark—which I have already elsewhere cited—of Linnæus's contemporary, Maupertuis, then president of the Berlin Academy of Sciences. Maupertuis spoke of zoology; but we may generalize his observation: "All these treatises on plants and animals which we as yet have," he says (about 1750), "are —even the most methodical of them—no better than pictures pretty to look at; in order to make of natural history a veritable science, naturalists must apply themselves to researches which can make us acquainted not simply with the form of this or that organism, but with the general *processes* of nature in the production of organisms and the conservation of them." Towards making natural history a veritable science in this sense Linnæus did relatively little; but it is not quite true to say that he did nothing at all. Towards the discovery or the establishment of two generalized laws respecting the processes of nature in the production and the perpetuation of vegetal organisms Linnæus made some contribution; and of these something ought briefly to be said, the more because they are often neglected in the accounts of Linnæus's work.

1. Although, as has been remarked, the fact of sexuality in plants had been noted by a number of great naturalists before 1718, the doctrine was not, in Linnæus's youth, at all generally accepted. It was possible at the beginning of the eighteenth century for a botanist so eminent as Tournefort to combat and ridicule the idea; and for the Imperial Academy of Sciences of St. Petersburg, so late as 1759, to offer a prize for the best argument either *for or against* the doctrine of sex in vegetables. Linnæus gave the weight of his authority, as well as of some new experimental evidence, to the affirmative of this question. By him the fact may be said to have been finally established; and by his sexual system of classification the idea was made a familiar and fundamental common-place of even popular botanical knowledge.

2. By his doctrine of the "Prolepsis Plantarum" and "Metamorphosis Plantarum"—which one of his disciples declared to be "the most subtle discovery of any which can be put forward by the investigators of nature in our age," but which there lacks space to set forth

in its details—Linnæus began that theoretical reduction of the several parts of a plant to modifications, under special conditions, of a few simple organs, which Goethe was to elaborate and carry much farther in his "Metamorphose der Pflanzen" (1790). Goethe makes due acknowledgment of his debt to Linnæus (who was his constant study in his early years[14]) in that treatise, the place of which in the history of botany is well known. Contemporary botanists would, I suppose, incline to question whether this theory has done greater service or harm to the progress of their science. Its chief value lay in its tendency to suggest the idea of the unity of type—and eventually the idea of the common derivation through processes of transformation—of different species. Both of these ideas were far from the mind of Linnæus; with him the theory took the form only of the purely specific doctrine of the interchangeability of leaf and flower under varying conditions of nourishment, or at different phases of the individual plant's growth.

In these two instances, then, Linnæus made some contribution to the unification, as well as to the augmentation, of knowledge. Yet his lack of any penetrating insight into the larger relations of biological facts and the absence in him of any sound grasp of scientific method, disqualified him from taking a place among those who have materially enriched our stock of the ideas and categories which may be used in the interpretation of nature. His emphasis upon the static aspects of the world of living organisms—upon the fixed characters of species—and upon the descriptive rather than explanatory business of scientific inquiry made his influence, on the whole, an obstacle to the development and diffusion of those evolutional ideas which were already stirring in a number of minds of his generation. His ineptitude in the more philosophic part of the naturalist's work could not be better shown than in the one treatise in which he attempts a broad philosophical view and a wide correlation of organic phenomena. This writing, "Œconomia Naturæ," which was greatly admired by his contemporaries, points out in how diverse and complicated ways organisms of different species interact with one another, and are reciprocally adapted to one another, as well as to the conditions of survival in their environment. In dilating upon this Linnæus may be said to call attention, more than a century before Darwin, to the reality and importance in nature of the struggle for existence between species; for he shows how every kind of organism has its natural enemies, with which it keeps up

[14] The poet himself wrote in his "Geschichte meines botanischen Studiums" (1817): "After Shakespeare and Spinoza, it was Linnæus who had the greatest influence upon me—chiefly, indeed, by the opposition that he provoked. For when I strove to make my own his sharp, clear-cut divisions and his apt and serviceable but often arbitrary laws, an inner conflict arose in me: what he sought forcibly to hold apart, the deepest need of my nature made me wish to bring back to unity."

an unceasing warfare or competition, as a result of which the otherwise excessive multiplication of each kind is prevented and the equilibrium of nature is preserved. But all these just observations lead Linnæus to nothing more useful to science than the *quam pulchre!* We are invited to see in the arrangement whereby the lion saves the lamb from the Malthusian inconvenience of over-multiplication simply an evidence of design in nature. It never occurs to the great naturalist to consider that, as Maupertuis put it, "since only those creatures *could* survive in whose organization a certain degree of adaptation was present, there is nothing extraordinary in the fact that such adaptation is found in all the species that now exist." Looking upon the same general class of facts as those which were to be considered by Wallace and Darwin, Linnæus finds in them nothing but the occasion for the wholesale introduction of teleological considerations, in place of causal explanations. In setting the example of such a proceeding, Linnæus certainly did much to hold biology back from its proper methods and its proper problems. In this, as in his general failure to take a philosophic view of his subject, his mental attitude was peculiarly uncongenial to the greatest intellect—if not the greatest botanist—of those whom he largely influenced. Goethe kept up a lifelong protest against all purely descriptive science and all introduction of teleological notions into the explanation of natural phenomena. And it is from Goethe in his old age that I may, in closing, quote a somewhat severe, but not unilluminating, remark upon the master of the poet's early botanical studies;[15] since it contains a sort of philosophical pun, it is necessary to give it in the German:

Eine zwar niedere doch schon ideelle Unternehmung des Menschen, ist das Zählen, wodurch im gemeinen Leben so vieles verrichtet wird; die grosse Bequemlichkeit jedoch, die allgemeine Fasslichkeit und Erreichbarkeit giebt dem Ordnen auch in den Wissenschaften Eingang und Beifall. Das Linnésche System erlangte eben durch diese Gemeinheit seine Allgemeinheit; doch widerstrebte es einer höheren Einsicht mehr, als dass es solche förderte.

Yet if Linnæus was not qualified to lead biology into the promised land of that "higher insight"—if he even somewhat delayed its progress thither—it must still be said that he left all the sciences with which he dealt incomparably better provisioned for that progress than they would have been without his work. He left to them an intensified ardor for the scrutiny of all the phenomena of nature, a better command of their own materials, and a greatly enriched and better ordered store of those concrete facts out of which, in time, scientific generalizations often almost spontaneously develop, and by which they must always eventually be tested.

[15] "Aphoristisches," Weimar-Ausg., Teil II., Bd. 6, § 356 (1829); cited by Wasielewski in his "Goethe und die Descendenzlehre."

THE PROBLEM OF AGE, GROWTH AND DEATH

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VI. THE FOUR LAWS OF AGE

Ladies and Gentlemen: I have referred in these lectures repeatedly to the cell and its two component parts, the nucleus and the protoplasm. To-night I shall have only a few references to make directly to these, and shall pass on for the latter part of the hour to another class of considerations bearing upon the problem of age. Before we turn to these new considerations, however, I wish to say a few words by way of recapitulation concerning the changes in the cells as corresponding to age. Cells, as you know from what I have told you, undergo in the body for the greater part a progressive change which we call their differentiation. We may say that there are four kinds of cells for purposes of an elementary classification to be used in a simple exposition like the present. The first kind are those cells of the young type, in which the protoplasm is simple, and shows as yet no trace of differentiation. These cells are capable of rapid multiplication, and some of them are found still persisting in various parts of the adult body, and serve to maintain the growth of the body in its mature stage. Another class of cells presents to us the curious spectacle of a partial differentiation; such are the muscle fibers by which we accomplish our voluntary movements. These fibers consisted originally only of protoplasm with the appropriate nuclei, but, as they are differentiated, part of the protoplasm changes into contractile substance. Another part remains pure protoplasm unaltered. If now the muscular or contractile portion of the fiber be destroyed, the undifferentiated part of the protoplasm then shows that it has still the power of growth. It has only been held back by the condition of organization, and we see in the regeneration of these fibers evidence of the fact that so long as the protoplasm is undifferentiated it has the power of growth, which, however, does not reveal itself unless an opportunity is afforded. Third, we come to the cells which are moderately differentiated; such, for instance, are the cells of the liver, and, if for any reason a portion of the liver be injured by accident or disease, we find that these partially differentiated cells reveal at once that they have a limited power of growth still left. If we pass on to the fourth class, that in which differentiation is carried to the highest extreme, we find that the cells do not have the power of multiplication. Such are the

nerve cells by which the higher functions of the body are carried on. They represent the extreme of cellular differentiation, and almost never do we see these cells multiplying after the differentiation is accomplished. Presented in this form, we then recognize, it seems to me clearly, the effect of differentiation upon the growth of cells. The facts are clear as to their meaning.

We can, however, proceed a little farther than this, because we can actually determine, approximately at least, the rate at which cells multiply, and that we can do by means of determining the mitotic index. The mitotic index is the number of cells to be found at any given moment in the active process of division out of a total of one thousand cells.

May I pause a moment to recall this picture to you and ask you to notice at this point the curious darker spot which represents a nucleus in process of division? You will see it would be easy in such a preparation as this to count the nuclei one by one until one had got up to a thousand, and to record, as one went along, how many of the nuclei are in process of division, for the nucleus in division is easily recognized. This process of division is named mitosis: the figure which the nucleus presents while it is undergoing division we call a mitotic figure. Counting the dividing nuclei, we may determine that in a thousand cells there are a given number which have nuclei in process of division, and such a number I propose to call "the mitotic index." I wish now only to call to you attention this picture because it enables me to illustrate before you the method of measuring the mitotic index.

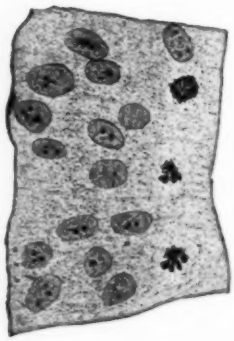

FIG. 61. PORTION OF THE OUTER WALL OF A PRIMITIVE MUSCULAR SEGMENT OF A CAT EMBRYO OF 4.6 MM. Harvard Embryological Collection Series 398, section 115. The resting nuclei are oval, pale and granular. The dividing or mitotic nuclei, of which there are three, are dark, irregular in outline and show the chromosomes. In this case the dividing nuclei all lie near the *inner* surface of the wall. The picture illustrates the ease with which mitotic figures may be recognized.

In the rabbit embryo at seven and one half days, I have found by actual count that there are in the outer layer of cells, known technically as the ectoderm, 18 of these divisions per thousand. In the middle layer, technically the mesoderm, 17, and in the inner layer, the entoderm, 18. At ten days we find the number already reduced, and the figures are, respectively, 14, 13 and 15, and for the cells of the blood only 10. There has already been a great reduction. In the next phase of development (rabbit embryo of thirteen days), we find, however, that the parts are growing irregularly, some faster, some slower. We note that wherever a trace of differentiation has occurred, the rate of growth is diminished: where that differentiation does not show itself, the

rate of growth may even increase in order to acquire a certain special development of a particular part. So that instead of uniformity of values for the mitotic index, we get a great variety. But, nevertheless, the general decline can be demonstrated by the figures. In the spinal cord the index is 11, in the general connective tissue of the body 10; for the cells of the liver 11; in the outside layer of the skin 10; in the excretory organ 6; in the tissue which forms the center of the limb also 6. There has, then, been a rapid decline in the rate of cell multiplication just in this period when differentiation is going on. This is, so far as I know, an entirely new line of research. The counting of a thousand cells is not a thing to be done very rapidly; it must be undertaken with patience, care, and requires time. It has not, I regret to say, been possible for me yet to extend the number of these counts beyond those I have given you, but it is easy to say that in the yet more differentiated state, the number of cells in division is constantly lessened, and it is only a question of counting to determine the mitotic index accurately. That there is a further diminution beyond that which the mitotic indices I have demonstrated to you represent is perfectly certain. I only regret that I am not able to give you exact numerical values.

I wish very much that my time permitted me to branch off into certain topics intimately associated with the general theme we have been considering together on these successive evenings, but we can only allude to a few of these. The first collateral subject on which I wish to speak to you briefly is that which we call the law of genetic restriction, which means that after a cell has progressed and is differentiated a certain distance, its fate is by so much determined. It may from that pass on, turn in one direction or another, always progressing, going onward in its cytomorphosis; but the general direction has been prescribed, and the possibilities of that cell as it progresses in its development become more and more restricted. For instance, the cells which are set apart to form the central nervous system after they are so set apart can not form any other kind of tissue. After the nervous system is separated in the progress of development from the rest of the body, its cells may become either nerve cells proper or supporting cells (neuroglia), which latter never acquire the nervous character proper, but serve to uphold and keep in place the true nervous elements. They represent the skeleton of the central nervous system. After the cells of the nervous system are separated into these two fundamental classes they can not change. A cell forming a part of the supporting framework of the brain can not become a nerve cell; and a nerve cell can not become a supporting cell. The destiny of them becomes more and more fixed, their future possibilities more and more limited, as their cytomorphosis goes on.

The law of genetic restriction has a very important bearing upon questions of disease. When disease occurs, the cells of the body offer

to us two kinds of spectacles. Sometimes we see that the cells causing the diseased condition are more or less of the sort which naturally belong in the body; that they are present where they do not belong, or they are present where they ought to be, but in excessive quantity. There is a kind of tumor which we call a bony tumor. It consists of bone cells such as are naturally present in the body, but that which makes this growth of bone a tumor is its abnormal dimensions, or perhaps its being altogether in the wrong place. The second sort of pathological alteration, which I had in mind, is that in which the cells really change their character. Now, the young cells are those which can change most; in which the genetic restriction has least come into play; and accordingly we find that a large number of dangerous, morbid growths, tumors, arise from cells of the young type, and these cells, having an extreme power of multiplication, grow rapidly, and they may assume a special character of their own; their genetic restriction has not gone so far that all their possibilities of change in the way of differentiation have been fixed; there is a certain range of possibilities still open to them, and they may turn in one direction or the other. Hence there may be pathological growths of a character not normally present in the body. It seems to me, so far as my knowledge of this subject enables me to judge, to be true that all such pathological growths depend upon the presence of comparatively young and undifferentiated cells being turned into a new direction. The problem of normal development and of abnormal structure is one and the same. Both the embryologist and the anatomist, on the one hand, and the pathologist and the clinician on the other, deal ever with these questions of differentiation, and practically with no others. All that occurs in the body is the result of various differentiations, and whether we call the state of that body normal or pathological matters little; still the cause of it is the differentiation of the parts.

The second of the collateral topics which I should like briefly to allude to is another branch of the study of senescence. The fact was first emphasized by the late Professor Alpheus Hyatt that in many animals there exist parts formed in an early stage and thereafter never lost. The chambered nautilus is an animal of this kind. The innermost chamber represents the youngest shell of the nautilus, and as its age increases, it forms a new chamber in its shell, and so yet more and more until the coil is complete. When we examine a shell of that kind we see permanently before us the various stages, both young and old, as recorded in shell formation. And so too in the sea-urchin, and in many of the common shell-fish, we find the double record, of youth and old age, preserved permanently. This has made it possible for Professor Hyatt and for Professor Robert T. Jackson, who has adopted a similar guiding principle, to bring a great deal of new light into the study of animal changes, and to attack the solution of problems which

without the aid of this senescent interpretation, if I may so term it, would be utterly impossible. This is an enticing subject, and I wish I had both time and competency to dwell upon it. But it is aside, as you see, from the main inquiries with which we have been occupied, for our inquiries concern chiefly the effect of cell-change upon the properties of the body, and the correlation of cell-change with age.

A natural branch of our topic is, however, that of longevity, the duration of life. Concerning this, we have very little that is scientifically satisfactory that we can present. We know, of course, as a fundamental principle, that every animal must live long enough to reproduce its kind. Did that not occur, the species would of course become extinct, and the mere fact that the species is existing proves, of course, this simple fact—that life has lasted long enough for the parents to produce offspring. The consideration of this fact has led certain naturalists to the supposition that reproduction is the cause of their termination of life; but it is not, it seems to me, at all to be so interpreted. We know, in a general way, that large animals live longer than small ones. The elephant is longer lived than the horse, the horse than the mouse, the whale than the fish, the fish than the insect, and so on through innumerable other instances. At first this seems a promising clue, but if we think a moment longer we recognize quickly the fact that a parrot, which is much smaller than a dog, may live one hundred years, whereas a dog is very old at twenty. There are insects which live for many years, like the seventeen-year locusts, and others which live but a single year or a fraction even of one year, and yet the long-lived and the short-lived may be of the same size. It is evident, therefore, that size is not in itself properly a measure of the length of life. Another supposition, which at first sounds very attractive, is that which explains the duration of life by the rate of wear, of the using up, of the wearing out, of the body. This theory has been particularly put forward by Professor Weismann, who in his writings calls it the *Abnutzungstheorie*—the theory of the wearing out of the body. But the body does not really wear out in that sense. It goes on performing the functions for a long time, and after each function is performed the body is restored, and we do not find at death that the parts have worn out. But, as we have seen, we do find at death that there has been an extensive cytomorphosis, cell-change, and that the living material, after having acquired its differentiation, passes now in one part, now in another, then in a third, to a yet further stage, that of degeneration, and the result of degeneration, or atrophy, as the case may be, is that the living protoplasm loses its living quality and becomes dead material, and necessarily the functional activity ceases. We must, it seems to me, conclude that longevity, the duration of life, depends upon the rate of cytomorphosis. If that cytomorphosis is

rapid, the fatal condition is reached soon; if it is slow, the fatal condition is postponed. And cytomorphosis in various species and kinds of animals must proceed at different rates and at different speeds at different ages. Birds grow up rapidly during their period of development; the cell change occurs at a high speed, far higher than that which occurs in man, probably, during his period of development. But after the bird has acquired its mature development, it goes on almost upon a level for a long time; the bird which becomes mature in a single year may live for a hundred or even more. There can be during these hundred years but a very slow rate of change. But in a mammal, a dog or a cat, creatures of about the same bulk as some large birds, we find that the early development is at a slower rate. The animals take a much longer period to pass through their infancy and reach their maturity, but after they have reached their maturity they do not sustain themselves so long. Their later cytomorphosis occurs at a higher speed than the bird's. This is a field of study which we can only recognize the existence of at present, and which needs to be explored before, to any general, or even to a special scientific, audience, any promising hypotheses can be presented. Definite conclusions are of course still more remote.

Next as regards death. The body begins its development from a single cell, the number of cells rapidly increase, and they go on and on increasing through many years. Their whole succession we may appropriately call a cycle. Each of our bodies represents a cell cycle. When we die, the cycle of cells gives out, and, as I have explained to you in a previous lecture, the death which occurs at the end of the natural period of life is the death which comes from the breaking down of some essential thing—some essential group of members of this cell cycle; and then the cycle is broken up. But the death is the result of changes which have been going on through the successive generations of cells making up this cycle. There are unicellular organisms; these also die; many of them, so far as we can now determine, never have any natural death, but there are probably others in which natural death may occur. It is evident that the death of a unicellular organism is comparable to the death of one cell in our own bodies. It is not properly comparable to the death of the whole body, to the ending-up of the cell cycle. Is there anything like a cell cycle among the lower organisms? among the protozoa, as the lowest animals are called? It has been maintained by a French investigator, by the name of Maupas, that such a cycle does exist, that even in these low organisms there is a cell which begins the development, and that gradually the loss in the power of cell multiplication goes on until the cycle gives out and has to be renewed by a rejuvenescent process, and this rejuvenating process he thinks he has found in the so-called conjugating act of these animals, in which there occurs a curious migration of the nucleus of

one individual into the cell body of another. Whether he is right or not remains still to be determined. You will recognize, I hope, from what I have said, that we have now some kind of measure of what constitutes old and young. We can observe the difference in the proportion of protoplasm and nucleus, the increase or diminution, as the case may be, of one or the other. If it be true that there is among protozoa, among unicellular animals, anything comparable to the gradual decline in the growth power which occurs in us, we shall expect it to be revealed in the condition of the cells—to see in those cells which are old an increase in the proportion of protoplasm, and consequently a diminution in the relative amount of nucleus. That subject is now being investigated, and we shall probably know, within a few years at least, something positive in this direction. At present we are reduced to posing our question. We must wait patiently for the answer.

The scientific man has many occasions for patience. He has to make his investigations rather where he can than where he would like to. Certain things are accessible to our instruments and methods of research at the present time, but other things are entirely hidden from us and inaccessible at the present. We are indeed, more perhaps than people in any other profession of life, the slaves of opportunity. We must do what we can in the way of research, not always that which we should like most to do. Perhaps a time will come when many of the questions connected with the problems of growing old, which we can now put, will be answered, because opportunities, which we have not now, will exist then. Scientific research offers to its devotees some of the purest delights which life can bring. The investigator is a creator. Where there was nothing he brings forth something. Out of the void and the dark, he creates knowledge, and the knowledge which he gathers is not a precious thing for himself alone, but rather a treasure which by being shared grows; if it is given away it loses nothing of its value to the first discoverer, but acquires a different value and a greater usefulness that it adds to the total resources of the world. The time will come, I hope, when it will be generally understood that the investigators and thinkers of the world are those upon whom the world chiefly depends. I should like, indeed, to live to a time when it will be universally recognized that the military man and the government-maker are types, which have survived from a previous condition of civilization, not ours; and when they will no longer be looked upon as the heroes of mankind. In that future time those persons who have really created our civilization will receive the recognition which is their due. Let these thoughts dwell long in your meditation, because it is a serious problem in all our civilization to-day how to secure due recognition of the value of thought and how to encourage it. I believe every word spoken in support of that great recognition which is due

to the power of thought is a good word and will help forward toward good results.

In all that I have said, you will recognize that I have spoken constantly of the condition of the living material. If it is in the young state it has one set of capacities. If it is differentiated, it has, according to the nature of its differentiation, other kinds of capacities. We can follow the changing structure with the microscope. We can gain some knowledge of it by our present chemical methods. Fragmentary as that knowledge is, nevertheless, it suffices to show to us that *the condition of the living material is essential and determines what the living material can do.* I should like to insist for a moment upon this conception, because it is directly contrary to a conception of living material which has been widely prevalent in recent years, much defended and popularly presented on many different occasions. The other theory, the one to which I can not subscribe, may perhaps be most conveniently designated by the term—the theory of life units. It is held by the defenders of this faith that the living substance contains particles, very small in size, to which the vital properties are especially attached. They look at a cell and find that it has water, or water containing a small amount of salts in solution, filling up spaces between the threads of protoplasm. Water is not alive. They see in the protoplasm granules of one sort and another, in plants chlorophyll, in animals perhaps fat or some other material. That is not living substance, and so they go striking out from their conception of the living material in the cell one after another of these component parts until they get down to something very small, which they regard as the life unit. I do not believe these life units exist. It seems to me that all these dead parts, as this theory terms them, are parts of the living cell. They are factors which enable the functions of life to go on. Other conditions are also there, and to no one of them does the quality of life properly attach itself. Of life units there is an appalling array. The most respectable of them, in my opinion, are the life units which were hypothetically created by Charles Darwin in his theory of pangenesis. He assumed that there were small particles thrown off from different portions of the body circulating throughout the body, gathering sometimes in the germ cells. These particles he assumed to take up the qualities of the different parts of the body from which they emanated, and by gathering together in immense numbers in the germ cells they accomplished the hereditary transmission. We know now that this theory is not necessary, that it is not the correct theory. But at the time that Darwin promulgated it, it was a perfectly sound defensible theory, a theory which no one considering fairly the history of biological knowledge ought to criticize unfavorably. It was a fine mental achievement, but I should like also to add that of all the many theories of life units, this of Darwin's is the only one which seems to

me intellectually entirely respectable. Of supposed structural life units there is a great variety. Besides the gemmules of Darwin, there were the physiological units of Herbert Spencer. Professor Haeckel, the famous German writer, has special structural life units of his own which he terms plastidules; he gave them the charming alliterative title of perigenesis of the plastidules; the rhythm of it must appeal to you all, though the hypothesis had better be forgotten. Then came Nägeli, the great botanist, who spoke of the Idioplasma-Theilchen. Then Weisner, also a botanist, who spoke of the Plassomes. Our own Professor Whitman attributed to his life units certain other essential qualities and called them idiosomes. A German zoologist, Haacke, has called them gemmules. Another German writer, a Leipzig anatomist, Altmann, calls them granuli. Now these different life units, of which I have read you briefly the names, are not identical according to these authors. Everybody else's life units are wrong, falsely conceived, and endued with qualities which they do not combine. There is a curious assemblage here of doxies, and each writer is orthodox and all the others are heterodox; and I find myself viewing them all from the standpoint of my doxy, that of the structural quality of the living matter, and, therefore, interpreting every one of these conceptions as heterodox, not sound doctrine, but something to be rejected, condemned and fought against. These theories of life units have filled up many books. Among the most ardent defenders of the theory of life units is Professor Weismann, whose theories of heredity many of you have heard discussed; though I doubt if many of you, unless you recall what I said previously, are aware of the fact that the essential part of Weismann's doctrine was the discovery of the theory of germinal continuity by Professor Nussbaum, whose name is seldom heard in these discussions. Weismann has gone much farther in the elaboration of the conception of life units than any of the other writers. He thinks the smallest of the life units are biophores. A group of biophores brought together constitutes another order of life units which he calls determinants; the determinants are again grouped and form ids; and the ids are again grouped and form idants. If you want to accept any theory of life units, I advise you to accept that of Weismann, for it offers a large range for the imagination, and has a much more formidable number of terms than any other.

I want to pass now to an utterly different line of study, the question of psychological development. If it be true that the development is most rapid at first, slower later, we should expect to find proof of that rate in the progress of mental development. In other words, we should expect to find that the baby developed faster than the child mentally, that the child developed faster than the young man, and the young man faster than the old. And do you not all instinctively feel immediately that the general assertion is true? In order, however, that

you may more fully appreciate what I believe to be the fact of mental development going on with diminishing rapidity, I should like to picture to you briefly some of the things which the child achieves during the first year of its life. When the child is born, it is undoubtedly supplied with a series of the indispensable physiological functions, all those which are concerned with the taking in and utilizing of food. The organs of digestion, assimilation, circulation and excretion are all functionally active at birth. The sense organs are also able to work. Sense of taste and of smell are doubtfully present. It is maintained that they are already active, but they do not show themselves except in response to very strong stimulation. Almost the only additional faculty which the child has is that of motion, but the motions of the new-born baby are perfectly irregular, accidental, purposeless, except the motions which are connected with the function of sucking, upon which the child depends for its nourishment. The instinct of sucking, the baby does have at birth. It might be described as almost the only equipment beyond the mere physiological working of its various organs. But at one month we find that this uninformed baby has made a series of important discoveries. It has learned that there are sensations, that they are interesting; it will attend to them. You all know how a baby of one month will stare; the eyes will be fastened upon some bright and interesting object. At the end of a month the baby shows evidences of having ideas and bringing them into correlation, association, as one more correctly expresses it, because already after one month, when held in the proper position in the arms, it shows that it expects to be fed. There is, then, already evidence and trace of memory. At two months much more has been achieved. The baby evidently learns to expect things. It expects to be fed at certain times; it has made the great discovery of the existence of time. And it has made the discovery of the existence of space, for it will follow, to some extent, the bright light; it will hold its head in a certain position to catch a sound apparently from one side; or to see in a certain direction. The sense of space and time in the baby's mind is, of course, very imperfect, doubtless, at this time, but those two non-stuff realities about which the metaphysicians discuss so much, the two realities of existence which are not material, the baby at this time has discovered. Perhaps, had some great and wonderfully endowed person existed who preserved the memory of his own psychological history, of his development during babyhood, we should have been spared the gigantic efforts of the metaphysicians to explain how the notions of space and time arose. Without knowing how, the baby has acquired them, and has already become a rudimentary metaphysician. We see, also, at the end of the third month, that the baby has made another remarkable discovery. It has found not merely that its muscles will contract and jerk and throw its parts about, which is doubtless earlier a great delight to it; but that

the muscles can contract in such a way that the movement will be directed; there is a coordination of the muscular movements. I should like to read to you just these three or four lines from Miss Shinn, who has given perhaps the best story of the development of a baby which has yet been written. This is not merely my opinion, but also the opinion of my psychological colleagues at Cambridge whom I consulted before venturing to express the idea before you, and I find that they take the view that Miss Shinn's book, which is charmingly written, is really done with such precision and understanding of the psychological problems involved that it may fairly be called the best of the books treating of the mental development of a baby. Miss Shinn says, referring to the condition of the child at the end of two months—"Such is the mere life of vegetation the baby lived during the first two months; no grown person ever experienced such an expansion of life—such a progress from power to power in that length of time." She is not thinking of senescence, as we have been thinking of it, but she makes precisely the assertion, which seems to me to be true, that the baby in two months has accomplished an amount of development which no adult is capable of. And now at three months we find another great discovery is made by the baby, that it is possible to bring the sensations which it receives into combination with the movements which it makes. It learns to coordinate its sensory impressions and its motor responses. We hardly realize what a great rôle this adjustment, between what our muscles can do and what our senses tell us, plays in our daily life. It is the fundamental thing in all our daily actions, and though by habit we perform it almost unconsciously, it is a thing most difficult to learn. Yet the baby has acquired the art, though he only gradually gets to be perfect in it. Again we see, at the end of the fourth month, that the baby begins to show some idea of another great principle—the idea that it can do something. It shows evidence of having purpose in what it does. Its movements are no longer purely accidental. At four months we find yet another equally astonishing addition to the achievements of this marvelous baby. He makes the amazing discovery that the two sides of an object are not separate things, but are parts of the same. When a face, for instance, disappears by a person's turning around, that face, to a baby of one month, probably simply vanishes, ceases to exist: but the baby at four months realizes that the face and the back of the head belong to the same object. He has acquired the idea of objects existing in the world around him. That is an enormous achievement, for this little baby has no instructor; he is finding out these things by his own unaided efforts. Then at five months begins the age of handling, when the baby feels of everything. It feels urgently of all the objects which it can get hold of and perhaps most of all of its own body. It is finding that it can touch its various parts and that when its hands and parts of its own body come in contact it has the double

sensation, and learns to bring those together and thereby is manufacturing in its consciousness the conception of the *ego,* personal, individual existence, another great metaphysical notion. Descartes has said—*Cogito, ergo sum*—I think, therefore I am. The baby, if he had written in Descartes's place, would have said—"I feel, therefore I am." The first five months constitute the first period of the baby's development. Its powers are formed, and the foundations of knowledge have been laid. The second period is a period of amazing research, constant, uninterrupted, untiring; renewed the instant the baby wakes up, and kept up until sleep again overtakes it. In the six months' baby we find already the notion of cause and effect. You see he is dealing mostly in metaphysical things, getting the fundamental concepts. That there is such an idea as cause and effect in the baby's mind is clearly shown by the progress of its adaptive intelligence. It evidently has now distinct purposes of its own. It shows clearly at this age also another thing which plays a constant and important rôle in our daily life. It has the consciousness of the possibilities of human intercourse; it wants human companionship. And with that the baby's equipment to start upon life is pretty well established. It has discovered the material universe in which it lives, the succession of time, the nature of space, cause and effect, its own existence, its ego and its relationship with other individuals of its own species. Do we get at any time in our life much beyond this? Not very much; we always use these things, which we learn in the first six months, as the foundation of all our thought. By eight months baby is upon the full career of experiment and observation. Everything with which the baby comes in contact interests him. He looks at it, he seizes hold of it, tries to pull it to pieces, studies its texture, its tensile strength, and every other quality it possesses. Not satisfied with that, he will turn and apply his tongue to it, putting it in his mouth for the purpose of finding out if it has any taste. In doing this, hour after hour, with unceasing zeal, never interrupted diligence, he rapidly gets acquainted with the world in which he is placed. At the same time he is making further experiments with his own body. He begins to tumble about; perhaps learns that it is possible to get from one place to another by rolling or creeping, and slowly he discovers the possibility of locomotion, which you know by the end of the year will have so far perfected itself that usually at twelve months the baby can walk. During this period of from five months to twelve the baby is engaged upon a career of original research, unaided much by anybody else, getting doubtless a little help and, of course, a great deal of protection, but really working chiefly by himself. How wonderful it all is! Is any one of us capable of beginning at the moment we wake to carry on a new line of thought, a new series of studies, and to keep it up full swing, with unabated pace, all day long till we drop asleep? Every baby does that every day.

When we turn to the child who goes to school, behold how much that child has lost. It has difficulties with learning the alphabet. It struggles slowly through the Latin grammar, painfully with the subject of geometry, and the older it gets, the more difficult becomes the achievement of its study. The power of rapid learning, which the baby has, is clearly already lessened.

The introduction of athletics affords a striking illustration of the decline of the learning power with the progressing years. When golf first came in it was considered an excellent game for the middle-aged; and you have all watched the middle-aged man play. He was so awkward, he could not do it. Day after day the man of forty, fifty, or even older, would go to the golf field, hoping each time to acquire a sure stroke, but never really acquiring it. The young man learned better, but the good golf players are those who begin as children, twelve and fourteen years of age, who in a few months become as expert and sure as their fathers wished to become, but could not. In bicycling it was the same. Eight lessons was considered the number necessary to teach the intelligent adult to ride a wheel. Three for a child of eight. And an indefinite number of lessons, ending in failure, for a person at seventy. It would have been scientifically interesting to have kept an exact record of the period of time which it took at each age to learn bicycling, but I think enough has been said to convince you that if we could acquire such a measure of psychological development as would enable us to express its rate in figures, we should be able to construct a curve like the curve which I showed you in the third lecture illustrating the decline in the rate of growth, and we should see that during the early years of life, the decline in the power of learning was extremely rapid, during childhood less rapid, during old age very slow. But the great part of the decline would occur during early years.

Here we see the principle of stability, in maturity, which we see also illustrated in structure and growth. The mind acquires its development; it retains that development in the adult a long time. But surely there comes a period when the exercise of the mind is difficult. It requires a great effort to do something new and unaccustomed. A sense of fatigue overwhelms us. I believe that this principle of psychological development, paralleling the career of physical development, needs to be more considered in arranging our educational plans. For if it be true that the decline in the power of learning is most rapid at first, it is evident that we want to make as much use of the early years as possible—that the tendency, for instance, which has existed in many of our universities, to postpone the period of entrance into college, is biologically an erroneous tendency. It would be better to have the young man get to college earlier, graduate earlier, get into practical life or into the professional schools earlier, while the power of learning is greater.

Do we not see, in fact, that the new ideas are indeed for the most part the ideas of young people. As Dr. Osler, in that much-discussed remark of his, has said, the man of forty years is seldom the productive man. Dr. Osler also mentioned the amiable suggestion of Trollope in regard to men of sixty, which has been so extremely misrepresented in the newspaper discussions throughout the country, causing biologists much amusement. But I think that Dr. Osler probably took a far too amiable view of mankind, and that in reality the period when the learning power is nearly obliterated is reached in most individuals very much earlier. As in every class of biological facts, there is here the principle of variation to be kept in mind. Men are not alike. The great majority of men lose the power of learning, doubtless some more and some less, we will say, at twenty-five years. Few men after twenty-five are able to learn much. They become day laborers, mechanics, clerks of a mechanical order. Others probably can go on somewhat longer, and obtain higher positions; and there are men who, with extreme variations in endowment, preserve the power of active and original thought far on into life. These of course are the exceptional men, the great men.

We have lingered so long together studying phenomena of growth, that it is natural to allude to one more, which is as singular as it is interesting, namely, the increase in size of Americans. It was first demonstrated by Dr. Benjamin A. Gould in his volume of statistics derived from the records of the Sanitary Commission—a volume which still remains the classic and model of anthropometric research. Any one, however, can observe that the younger generation of to-day tends conspicuously to surpass its parents in stature and physical development. How to explain the remarkable improvement we do not know. Our discovery of the fact that the very earliest growth is so enormously rapid, makes that earliest period especially important. If the initial growth can be favored a better subsequent development presumably would result. In brief, I find myself led to the hypothesis that the better health of the mothers secures improved nourishment in the early stages of the offspring, and that the maternal vigor is at least one important immediate cause of the physical betterment of the children. Much is said about the degeneracy of the American race, but the contrary is true—the American race surpasses its European congeners in physical development.

You will naturally wish to ask, before I close the series of lectures, two questions. One, how can rejuvenation be improved; the other, how can senescence be delayed. These questions will strike every one as very practical. But the first, I fear, is not an immediately practical question, but rather of scientific interest, for we must admit that the production of young individuals is, on the whole, very well accomplished and much to our satisfaction. But in regard to growing old,

in regard to senescence, the matter is very different. There we should, indeed, like to have some principle given to us which would delay the rate of senescence and leave us for a longer period the enjoyment of our mature faculties. I can, as you have readily surmised by what I have said to you, present to you no new rule by which this can be accomplished, but I can venture to suggest to you that in the future deeper insight into these mysteries probably awaits us, and that there may indeed come a time when we can somewhat regulate these matters. If it be true that the growing old depends upon the increase of the protoplasm, and the proportional diminution of the nucleus, we can perhaps in the future find some means by which the activity of the nuclei can be increased and the younger system of organization thereby prolonged. That is only a dream of the possible future. It would not be safe even to call it a prophecy. But stranger things and more unexpected have happened, and perhaps this will also.

I do not wish to close without one added word. The views which I have presented before you in this series of lectures I am personally chiefly responsible for. Science consists in the discovery made by individuals, afterwards confirmed and correlated by others, so that they lose their personal character. The views which I have presented to you, you ought to know are still largely in the personal stage. Whether my colleagues will think that the body of conceptions which I have presented are fully justified or not, I can not venture to say. I have to thank you much, because between the lecturer and his audience there is established a personal relation, and I feel very much the compliment of your presence throughout this series of lectures, and of the very courteous attention which you have given me.

To recapitulate—for we have now arrived at the end of our hour—we may say that we have established, if my arguments before you be correct, the following four laws of age.

First, rejuvenation depends on the increase of the nuclei.

Second, senescence depends on the increase of the protoplasm, and on the differentiation of the cells.

Third, the rate of growth depends on the degree of senescence.

Fourth, senescence is at its maximum in the very young stages, and the rate of senescence diminishes with age.

As the corollary from these, we have this—natural death is the consequence of cellular differentiation.

RADIOACTIVITY OF ORDINARY SUBSTANCES

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DURING the latter part of the nineteenth century a great deal of work was done upon electrical discharges in rarefied gases. In 1895 Röntgen made the epoch-making discovery that such a discharge was the source of very penetrating radiations. These radiations he called X-rays on account of their unknown nature, and he found that they possessed the power of making a gas a conductor of electricity by producing in it a great number of positively and negatively charged carriers or ions. Besides ionizing a gas, the X-rays were found to affect a photographic plate just as light rays do and to be able to penetrate thin sheets of the metals and many other bodies which are opaque to light. It was found in the course of experimentation that these X-rays were closely related to the stoppage of the cathode particles or corpuscles, and the phosphorescence on the walls of the vacuum tube which these corpuscles excite. In 1897 J. J. Thomson found that these cathode particles or corpuscles were small negatively charged particles of an apparent mass only one seven-hundredth that of the hydrogen atom and that in a "high vacuum" tube in a strong electric field they acquired a velocity approximating that of light. All the properties of the corpuscles were found to be the same, no matter what kind of gas or electrodes were in the discharge tube. Their mass was found to vary with their velocity in such a way that the whole mass of the corpuscle could be ascribed to the electric charge which it carried. From this most important discovery it was concluded that all the common substances were partly made up of corpuscles, and this conclusion has been strengthened by all later discoveries. After Thomson's discovery, Stokes showed that the sudden stoppage of the corpuscles by the walls of the discharge tube caused intense electromagnetic disturbances to travel out from the point of impact. These disturbances are the X-rays and travel with the velocity of light.

Discovery of Radioactivity

When Röntgen announced his discovery, it created a great impetus in the study of everything related to electrical discharges. Now it had been known for a long time that some bodies like the uranium salts phosphoresce when exposed to sunlight, and it occurred to H. Becquerel that such a phosphorescing body might emit X-rays, this

emission being analogous to the origin of X-rays in the phosphorescing glass walls of a vacuum tube. In accordance with this view in 1896 he exposed a photographic plate to uranium sulphate which was covered with copper and aluminium foil and found that the plate was acted upon. Accidentally he found that this action took place, no matter whether the uranium nitrate was phosphorescing or not, and he found that uranium which had never been exposed to sunlight possessed the same property. He found that these radiations from uranium were similar to the X-rays in their penetrating power. This was the first discovery of the possession of radioactivity by a body, *i. e.*, the power of a body to ionize a gas, to affect a photographic plate or to produce phosphorescence.

Madame Curie then took up the problem of finding whether other substances possessed the properties of uranium and found that thorium did. She made a detailed investigation then of all the elements and found that none, with the exception of uranium and thorium, possessed these properties even to the order of the hundredth part of that of uranium. She, however, found that some minerals possessed a greater radioactivity than uranium or thorium, and concluded that these must contain elements more highly radioactive than either of these. After much tedious, but brilliant, work she was able to separate out the very radioactive element radium. As a result of the work of the Curies, and many others, it was found that thorium, uranium, radium and actinium were radioactive, the latter two being intensely so. None of the other elements were found to possess any radioactivity to within the limits of the experimental errors of the method of observation.

The study of these radioactive elements has been the source of very important discoveries in physics. These elements are found to emit spontaneously a continuous flight of material particles, projected with great velocity, and also to be the source of radiations similar to X-rays and called γ rays. We will now describe the material particles, which are of two kinds, the α and β rays.

The α rays consist of positively charged particles shot out by the radioactive body with a velocity approaching that of light. They are readily absorbed by thin sheets of metal foil or by a few centimeters of air. The β rays are far more penetrating in character than the α rays and consist of negatively charged bodies projected with velocities of the same order of magnitude as that of light. As far as known, they are identical with the corpuscles. Of the three kinds of rays, the α rays produce the greatest amount of ionization and the γ rays the least. With a thin layer of unscreened radioactive matter spread on the lower of two plates, say 5 cm. apart, it will be found that the relative order of ionization due to the α, β and γ rays is as 10,000 to 100 to 1, whereas the average penetrating power is inversely proportional

to the relative ionization. The photographic action is due almost entirely to β rays.

The Disintegration Theory

The radioactivity of the radio-elements is not a molecular, but an atomic, property, and the rate of emission of the radiations depends on the amount of the element present and is unaffected by the application of any known physical or chemical forces. In order to explain the emission of positively and negatively charged particles, Rutherford and others consider the radio-elements as undergoing spontaneous changes and that the energy of projection of the α and β rays had previously been stored up in the atom as rapid oscillatory or orbital motion. This breaking up of the atoms is considered to be accompanied by the production of a series of new substances which have distinct physical and chemical properties. For instance, thorium produces an intensely radioactive substance, thorium X, which is soluble in ammonia. Thorium gives rise also to a gaseous product, the thorium emanation, and this is the source of another substance, which is deposited on the surface of bodies in the neighborhood of the thorium, and which is known as the "excited activity" or the "active deposit." If a negatively charged wire is brought into close proximity with thorium salts, the "active deposit" will form upon it. The "active deposit" itself decays into a succession of products.

Following will be given some of the products of the various radioactive elements and some of their properties.

Table I.

Transformation Products. The Thorium Group

	Product	Time to be Half Transformed	Radiations	Range of α Rays	Physical and Chemical Properties
	Thorium	$2(10)^9$ yrs.	Rayless (?)		Insoluble in ammonia
	Radiothorium	?	α rays	3.9 cm.	
	Thorium X	3.6 days	"	5.7 cm.	Soluble in ammonia
	Emanation	54 secs.	"	5.5 cm.	Inert gas condensing at —120° C.
Active Deposit	Thorium A	11 hrs.	Rayless		The active deposit is concentrated on the cathode in an electric field.
Active Deposit	Thorium B	1 hr.	α rays	5.0 cm.	
Active Deposit	Thorium C	Very short	α, β, γ rays	8.6 cm.	

The Uranium Group

Uranium	$(10)^9$ yrs.	α rays	3.5 cm.	Soluble in an excess of ammonium carbonate
Uranium X	22 days	β & γ rays		Insoluble in ammonium carbonate.

THE ACTINIUM GROUP

	Actinium	?	Rayless		Insoluble in ammonia
	Radioactinium	19.5 days	α rays	4.8 cm.	Carried down in an actinium solution by adding $BaCl_2$
	Actinium X	10.2 days	"	6.55 cm.	Soluble in ammonia
	Emanation	3.9 secs.	"	5.8 cm.	Behaves like a gas
Active Deposit	Actinium A	36 min.	Rayless		The active deposit is concentrated on the cathode in an electric field
Active Deposit	Actinium B	2 min.	α, β, γ rays		

THE RADIUM GROUP

	Radium	1,300 yrs.	α rays	3.50 cm.	Allied chemically to barium
	Emanation	3.8 days	"	4.33 cm.	Inert gas of heavy molecular weight, condensing at —150° C.
Active Deposit on Rapid Change	Radium A	3 min.	"	4.83 cm.	The active deposit is concentrated on the cathode in the electric field
Active Deposit on Rapid Change	Radium B	26 min.	β rays of low penetrating power		
Active Deposit on Rapid Change	Radium C	19 min.	α, β, γ rays	7.06 cm.	
Active Deposit on Color Change.	Radium D or Radiolead	40 years	Rayless		Volatile below 1,000° C.
Active Deposit on Color Change.	Radium E	6 days	β rays		Non-volatile at 1,000° C.
Active Deposit on Color Change.	Radium F or polonium or radiotellurium	140 days	α rays	3.86 cm.	Deposited in bismuth in solution.

In connection with these tables it may be well to consider the α particles a little more. It has been found that the α particles of any one product are emitted with the same velocity. It is found that after passing through a definite distance of gas, they then cease to ionize it. If the gas is air under normal conditions of pressure and temperature, this distance will be the range of the α particle. All experiments up to the present indicate that the α particles of the different products differ only in the speed of projection, this speed determining the range of ionization. Rutherford has found an empirical relation between the range of the α particle and its velocity at any point in its path. If r is the remaining range after passing through a screen, its velocity is $V = .348\ V_0\ \sqrt{r} + 1.25$, where V_0 is the initial velocity of the α particles emitted from radium C, and is $2.06\ (10)^9$ cm. per sec. The initial velocity of expulsion of an α particle from a certain product will then be a constant. The value of e/m for all α rays measured has been found to be the same and to be about $5.07\ (10)^3$ electro-magnetic units. It thus follows that all the radio-elements possess the

common constituents, the α and β particles. It has been found that after a certain critical velocity has been reached, the α particles all at once cease to produce any ionization, phosphorescence or photographic action. If a substance emits particles with a velocity less than this critical velocity, we should have no method at present available for detecting them. As to the enormous amount of ionization produced by radium, one can partly grasp it when one considers that an α particle produces about 80,000 ions and one grm. of radium emits about $6\ (10)^{10}$ α particles per second. The γ rays also differ in penetrating power. The γ rays from radium and thorium are very much stronger than those from uranium and actinium.

The Relationship between Various Elements

By actual experiment in the laboratory it is possible to watch the gradual formation of helium and actinium. The rate of formation of helium from radium is known roughly, so that if in any rock the helium formed from radium has not been allowed to escape, a quite accurate estimation of the age of the rock can be made. Radium has also been found by Boltwood and Rutherford to grow in actinium solutions. But by investigating elements which appear together in the rocks it is possible to learn much more. In fact it was from the occurrence of helium in radioactive minerals that the brilliant prediction of the production of helium was made. Boltwood, Strutt and McCoy have shown that the amount of radium present in radioactive minerals always bears a constant ratio to the amount of uranium present. For every gram of uranium there is present $3.8\ (10)^{-7}$ grms. of radium. From this coexistence in a constant ratio, one is justified in assuming that radium is a product of uranium. If this is true it is easy to explain the existence of radium in rocks that contain uranium. Otherwise, on account of the short period of disintegration of radium, it would be difficult to account for its distribution through the rocks. It has also been found that minerals of the same age contain uranium and lead in the same ratio, so that it seems quite certain that lead is a disintegrated product resulting from radium. Recent experiments by Rutherford seem to indicate, however, that actinium is not a direct product of uranium, as radium is considered to be. The existence together of various other elements has been used as an argument for their relationship. At present, however, the evidences are very meager and often conflicting. This field of experiment is one that promises very important results, however.

The Distribution of Radium

Considerable work has recently been done by the Hon. R. J. Strutt upon the amount of radium contained in various kinds of rocks widely

distributed over the earth. As this kind of work will probably have considerable geological importance, a few of Strutt's results will be given. A solution of a definite amount of the rock was stored until the equilibrium amount of radium emanation had accumulated. Now, as we have seen that uranium and radium occur in a constant proportion, it is possible to use a mineral of known uranium content and to compare the amount of radium emanation emitted by this and that emitted by the given rock. Following are some of Strutt's results, using 3.8 $(10)^{-7}$ grms. of radium as accompanying 1 gram of uranium in uranium minerals.

TABLE II.

RADIUM CONTENT OF IGNEOUS ROCKS

Name of Rock	Locality	Density	Radium per Gram in Grams	Radium per c. c. in Grams
Granite	Rhodesia	2.63	4.91 $(10)^{-12}$	12.9 $(10)^{-12}$
"	Cornwall	2.62	4.80 $(10)^{-12}$	12.6 $(10)^{-12}$
Blue ground	Kimberly	3.06	1.73 $(10)^{-12}$	5.29 $(10)^{-12}$
Leucite basanite	Vesuvius	2.72	1.71 $(10)^{-12}$	4.66 $(10)^{-12}$
Hornblende granite	Assouan, Egypt	2.64	1.26 $(10)^{-12}$	3.32 $(10)^{-12}$
Hornblende diorite	Heidelberg	2.89	1.02 $(10)^{-12}$	2.94 $(10)^{-12}$
Augite syenite	Norway	2 73	.95 $(10)^{-12}$	2.60 $(10)^{-12}$
Granite	Isle of Rum	2.61	.37 $(10)^{-12}$	.97 $(10)^{-12}$
Basalt	Greenland	3.01	.31 $(10)^{-12}$	.94 $(10)^{-12}$
Native iron	"		.218 $(10)^{-12}$	
Meteoric iron	Thunda		Undetectable	
" "	Virginia		"	
" "	Santa Catarina		"	

RADIUM CONTENT OF SOME SEDIMENTARY ROCKS

Name of Rock	Locality	Radium Content per Gram in Grams
Oolite	Bath	3.00 $(10)^{-12}$
Marble	East Lothian	1.99 $(10)^{-12}$
Roofing slate	Wales	1.32 $(10)^{-12}$
Gault clay	Cambridge	1.09 $(10)^{-12}$
Clay	Essex	.89 $(10)^{-12}$
Red chalk	Hunstanton	.55 $(10)^{-12}$
White marble	India	.28 $(10)^{-12}$
Chalk	Cambridgeshire	.40 $(10)^{-12}$
Deposit from Bath hot springs		.425 $(10)^{-12}$
Cambridge tap water		.400 $(10)^{-12}$
Sea salt		.077 $(10)^{-12}$
Boiler crust, Cambridge		.040 $(10)^{-12}$
Sea salt	Omaha	.0204 $(10)^{-12}$
Sea water	Atlantic	.003 $(10)^{-12}$

These two tables give but a few of the analyses of Strutt. They show the very wide distribution of radium both in the igneous and in the sedimentary rocks. The average radium content of the rocks examined by Strutt is high, whereas that of sea salt is quite small. Strutt finds that the radium content necessary to maintain the earth at a constant temperature is about 1.75 $(10)^{-13}$ grms. of radium per cubic centimeter of the earth. This is very much less than the lowest radium content of any of the rocks. For this reason he believes that

radium is to be found only in an outer crust of the earth, at least if the earth is becoming cooler. In making these calculations, the effect of thorium and uranium and the possible radioactivity of ordinary materials is not taken into account. If the heating effect of ordinary materials is of the same order of magnitude as is to be expected from the ionization they produce, the earth's temperature gradient would be many times larger than that observed. Strutt believes this to be an argument that ordinary matter possesses no genuine radioactivity of its own. C. B. Thwing claims, however, that he has been able to find a temperature gradient in small cylinders of the various metals and rocks. At present nothing very definite can be said as to the heating effect of the radioactivity of substances upon the temperature of the earth. Having considered the radioactivity of the various rocks, we will now take up the atmosphere.

Radioactivity of the Atmosphere

It was found after considerable work had been done on ionization that the free air is very considerably ionized. Now in the table of the transformation products of the radio-elements, it will be noticed that several products have the property of condensing on a highly negatively-charged wire. Elster and Geitel and others tried exposing such a wire in the open air and found that there was an active deposit of radium and thorium formed on the wire. The amount of active deposit was found to depend upon the locality and the weather conditions. If the air had been undisturbed for some time as the air in caves and cellars, it was found that the active deposit formed was much greater. Air sucked through the pores of the ground was found to be very active. From these results, Elster and Geitel concluded that the radium and thorium emanations (which behave like gases) ooze up through the ground and percolating waters and have their origin in the radium and thorium in the soil. The emanation then breaks up into the various products as given in Table I. The emanation in this course gives rise to positively charged carriers, which are driven to a negatively charged wire by the electric field. It is to the emanation and its products that the ionization of the air is attributed. Thorium C and radium C give off γ rays, and, as these are very penetrating, they would be the source of a very penetrating radiation, and this latter has been discovered several years ago. The ionization in a closed electroscope is measured, and thick lead screens are then placed around the electroscope, and the ionization is again measured. The ionization in the latter case is found to be very considerably decreased, the penetrating radiation having been largely cut off. Whether all the penetrating radiation can be explained as due to radium C and thorium C will be taken up later.

As the potential of the earth is negative compared with that of the air, the active deposit is dragged down to the surface of the ground and upon the leaves and branches of plants and trees. A hill or mountain top concentrates the earth's field and so receives a greater amount of the active deposit. In this way Elster and Geitel explain the greater ionization on hills and mountains. Experiments show that the active deposit tends to collect on dust particles. These dust particles serve as nuclei for the condensation of raindrops and snowflakes. The deposit resulting from evaporating rain and snow should be very radioactive. This was found to be true by Wilson and Allen. Again, a big rain or snow should carry down most of the active deposit, and as the emanation does not emit γ rays, the amount of γ radiation from the radioactive matter in the air should be very much decreased. The penetrating radiation, if it consists mainly of γ rays, should then become very small. This has been found to be borne out by experiments made by the writer. It must be remembered that the emanation is insoluble in water and as this does not seem to be carried down by water or snow, the products radium C and thorium C would soon be in equilibrium again after the rain or snow.

The effect of the presence of radioactive matter in the atmosphere upon ordinary phenomena is perhaps very great, though at present little is known. It has been found that deep wells and hot springs contain considerable radium. From this Elster and Geitel suggest that the curative effect of thermal springs and the physiological action of the air at high levels may be related to the large amount of radioactive matter present. The presence of radioactive matter, and therefore of ionization, in the air probably plays a very important rôle in the growth of plants. It has been found that vegetables grown in an atmosphere electrified positively are much above those grown in normal fields both in quantity and in quality. The ionization and nucleation produced by radioactive matter in the air are very essential for the condensation of rain and hail, and serve to explain the enormous accumulation of static electricity during thunderstorms. Simpson and others have measured the activity of the air which has blown over the sea and have found it small. Now if most of the radium and thorium emanations come from the pores of the soil and underground cavities, the results obtained by the above investigators would be expected, for, as will be seen from Table II., the radium content of ocean water is very small. Eve has recently measured the ionization over the ocean and has found it to be the same as the ionization over the land, a rather unexpected result. In this state the matter rests at present. A crucial test would be to expose negatively charged wires far out in the ocean and find whether there was any active deposit and to test for the presence of a penetrating radiation. According to J. Joly, the distinguished geologist, Eve's results can easily be explained. Geologists have

for some time made an approximate estimate of the age of the oceans by making determinations of the amount of salt which they contain. By analyzing the waters of rivers flowing into the ocean for the salt which they contain and determining the total annual outflow of all the rivers into the ocean, and supposing these constants to have been practically constant during the past, it is easy to make an estimate of the approximate age of the oceans. Now if radium and uranium always exist in a constant proportion, the present radium content of the ocean would have been supplied by the rivers in a comparatively short length of time. For this and other reasons Joly believes that uranium and radium are not always to be found associated together. Now we know that radium has a short period of decay, so that it must constantly be supplied from somewhere. Joly believes that the source is at least partly outside of the earth. This radium is gradually being brought down to the surface. This would account for the ionization over the ocean and the wide distribution of radium over the earth. Elster and Geitel's theory of the escape of the emanation from the upper layers of the soil would still hold true. If radium exists outside the earth, it would be expected that the upper layers of the earth's atmosphere would be highly ionized by the γ rays. This highly ionizing radiation would serve to explain some of the phenomena of atmospheric electricity. According to C. T. R. Wilson, the positive potential of the atmosphere is largely to be attributed to the carrying down of negative charges by raindrops and snowflakes. The upper layers of the atmosphere, being highly ionized and quite good conductors, would conduct the remaining positive charge to places of lower potential and would thus always aid in equalizing the potential of wet and dry regions.

Radioactivity of the Metals

After the discovery that several of the elements were radioactive, it was natural to ask if radioactivity was a universal property of all the elements. Madame Curie's work showed that if the ordinary elements are radioactive at all, they must possess this property to but a very slight degree. In order to detect any possible radioactivity, it was necessary to have very sensitive instruments. It was found by Wilson and Geitel that there is a leakage of electricity through a gas in a closed vessel and that this leak could be measured very accurately by means of an electroscope. Now either the ions are produced spontaneously in the gas, by a radiation which is capable of penetrating the sides of the electroscope or by radiations from the walls of the electroscope itself. Rutherford, Cooke and McClennan have shown that some thirty or forty per cent. is due to a very penetrating radiation supposed to be the γ rays emitted by the radium and thorium products in the air and ground. By using lead screens around the electroscope, they were able to decrease the rate of leak to a certain limiting value

beyond which they were unable to go, no matter how much lead was used. Strutt and others then found that for electroscopes of the same dimensions, the amount of ionization depended on the material forming the walls. For vessels of the same shape and size, lead walls gave the greatest amount of ionization, tin and iron considerably less, aluminium and glass the least of all. Strutt found that different specimens of the same metal gave a different ionization and he therefore concluded that the radioactivity of the metals was probably due to a common impurity.

Patterson then tried using different gases and found that the ionization was proportional to the density. This fact is strong evidence that the ionization is not spontaneous within the gas, but is due to a radiation from the walls of the vessel. Patterson also found for the given vessel which he used (30 cm. in diameter and 20 cm. long) that the current through the gas was independent of the pressure above 300 mm. of mercury and varied directly as the pressure below 80 mm. The ionization was found independent of the temperature up to 450° C. That the ionization was related to the pressure as stated above would indicate that above 300 mm. of mercury all the radiation was absorbed, whereas below 80 mm. it was not all absorbed.

The most complete work on the radiations from the metals and their salts has been done by Campbell. In experiments on the radiations from the metals, Campbell used an aluminium-lined box. Inside this was a wire gauze cage containing a gauze electrode. The cage would allow the admission of radiations, but not of ions. Then by placing two sheets of metal so as to radiate into the cage, one sheet being arranged to slide back and forth, it was possible to measure the ionization produced at different distances of this sliding sheet from the cage. The curve which was plotted from the values of the ionization and the distances gave the values of various constants from which it was possible to determine the values given in a table which is shortly to follow. Before considering this table it is needful to say that the curves indicated (when the external penetrating radiation was cut off): (1) an easily absorbable radiation from the sheets of metal placed aside of the cage; (2) a more penetrating radiation from the same; and (3) the radiation from the gauze cage. When the external penetrating radiation was not screened off, the curves showed in addition an ionization due to (4) the external pentrating radiation; (5) to the penetrating radiation excited by it; and (6) to the easily absorbable radiation also produced, In the table, a is Bragg's constant for the intrinsic absorbable radiations, a constant which corresponds to the range of the α particles of the radioactive elements; s is the number of ions produced per second by the intrinsic absorbable radiation from one square centimeter of the surface of the metal, when totally absorbed in air; λ is the coefficient of absorption of the easily absorbable secondary radiation; s' is the number of ions produced per cubic centimeter by

the easily absorbable secondary radiation from one square centimeter of the metal under the circumstances of the experiment; V is the number of ions produced in 1 c.c. by the intrinsic penetrating radiation from the whole box and lead screen; V' is the number of ions produced in 1 c.c. by the external radiation and the penetrating radiation excited by it.

TABLE III.

Metal	s	s'	V	V'	a	λ
Lead (1)	270	0	10.2	14.2	12	
" (2)	260	0	13.4	26.3	12	
Copper (1)	103	160	2.2	22	9	.6
" (2)	110	91	8.1	27.4	9	.5
Aluminium	117	0	14.8	17	6	0
Tin	144	156	3.1	18.9	9	.5
Silver	146	146	25.5	17.0	8.5	.9
Platinum	74	411	17.3	14.1	12	.4
Gold	78	169	10.4	16.8	10	.6
Zinc	72	51	15.4	16.8	10	.5
Iron	119	124	12.3	10.5	13	.5

λ is the coefficient of absorption of the easily absorbable secondary radiation.

By using a strong electrostatic field, an attempt was made to determine whether the ionizing agents for the intrinsic absorbable radiation were charged. These radiations were found certainly not to be of the β type and very probably to have a nature similar to that of the α rays. No radium emanation was able to be detected from the lead used.

From the constancy of the value of s for the different specimens of the same metal and on account of the variation in the values of a for the different metals, Campbell rightly concludes that there seems to be no doubt but that the ordinary metals are feebly radioactive. In some cases the experimental and theoretical curves agree so well that it would seem that the radiations are homogeneous.

Campbell has also investigated the radioactivity of the metals and their salts in a similar way. He finds that the emission of radiations is an atomic property and that salts prepared by totally different methods and from materials derived from different sources, produce the same ionizing effect. It is only the metal that produces any measurable ionizing effect. The following are some of the results:

Substance	Activity in Arbitrary Units	Substance	Activity in Arbitrary Units
Lead	9.3	Tin	4.4
Lead sulphate (1)	6.8	Tin sulphide (1)	4.1
" " (2)	7	" " (2)	3.9
" " (3)	7.2	" " (3)	3.8
Lead monoxide	8.2	Bismuth	6
Mercury	.9	Bismuth oxide	5.7
Mercurous oxide	.5	Potassium sulphate	70
Mercuric oxide	.6		

Substance	Per Cent. Potassium	Activity of Salt	Activity of Potassium	Weight of Salt
Potassium sulphate (1)	44.7	489	1,090	220
" " (2)	"	471	1,050	215
Potassium chloride (1)	52.1	495	951	135
" " (2)	"	"	"	139
Potassium iodide	23.5	276	1,180	232
Potassium nitrate	38.6	388	1,005	199
Potassium sulphate (from wood)	44.7	474	1,060	
Orthodase	16.5	201	1,220	
Rubidium alum	16.6	128	768	

The use of numerals after the name of the substance is to indicate that the substance was made by a distinct method of analysis.

It will be noticed that the ionization from potassium and rubidium is very large compared with that from the other metals. It was found that the penetrating power of the potassium and rubidium radiations was also quite large. A given sample of a potassium salt gave the following results:

Number of Sheets of Foil	Ionization	Decrease
0	467	
1	361	106
2	299	62
3	265	34
4	240	25

It will be seen that the rays are very heterogeneous and vary in penetration from that of the very penetrating β rays of uranium downward. Sodium, lithium and ammonium salts showed no more activity than zinc. The rays from rubidium were found less penetrating than those from potassium. The activity of uranium is about a thousand times that of potassium. Photographs were also taken by making use of the rays from potassium and rubidium.

Campbell's results are in consonance with the experiments made some time ago by J. J. Thomson. Thomson showed that rubidium and potassium emit negatively charged particles which were deflected by an electrostatic field in the same way as the ordinary corpuscles.

Conclusion

In summing up we find that:

1. Some of the elements, as radium and thorium, are intensely radioactive.

2. Radium is very widely distributed through the rocks of the earth, and in radioactive minerals is found to exist in a constant proportion with uranium.

3. Radium and its products are also to be found in the air and play an important rôle in atmospheric phenomena.

4. The ordinary metals are slightly radioactive, emitting radiations that seem very much like the α radiation from the radio-elements.

5. Potassium and rubidium emit radiations similar to the β rays.

THE INFLUENCE OF DIET ON ENDURANCE AND GENERAL EFFICIENCY

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EXPERIMENTAL study of the physiological needs of the body for food[1] has indicated that the real requirements of the system, especially for proteid foods, are far below the amounts called for by existing dietary standards, and still farther below the customary habits of the majority of mankind. The ability of the body to maintain a condition of physiological equilibrium, with a true nitrogen balance, etc., on a relatively small amount of nitrogenous food, would seemingly imply that the large surplus so generally consumed constitutes an entirely uncalled-for drain upon the system, as well as upon the pocket of the individual, and without any compensatory gain.

In our experimental study of this question, observations on many individuals have extended over such long periods of time that there is apparently perfect safety in the conclusion that the new dietary standards which aim to conform to the true needs of the body are perfectly adapted to maintain health, strength and vigor indefinitely. Further, the many data obtained in our experimental studies, reinforced by a multitude of personal experiences from all over the world, communicated to the writer, all lead to the view that there is great personal gain in the acquisition of dietary habits that tend toward moderation and simplicity. Renewed health, increased vigor, greater freedom from minor ailments, etc., are so frequently reported as the outcome of temperance in diet, that we are forced to the conclusion that the surplus of proteid food so commonly consumed—amounts far beyond what the physiological necessities of the body demand—is wholly unphysiological and in the long run detrimental to the best interests of the individual. There is seemingly sound philosophy in so changing the customs and habits of our daily life that they will conform more or less closely to our present understanding of the physiological requirements of the body.

It is certainly not presumptuous to assume that physiological experimentation can tell us definitely and concisely how much and what kinds of food are needed to supply the daily waste of tissue and to make good the loss of energy incidental to varying degrees of bodily

[1] See Chittenden: "Physiological Economy in Nutrition" and "The Nutrition of Man," Frederick A. Stokes Co., New York.

activity. This we have sought to ascertain by our studies of the past five years, and confidence in our results is augmented by the fact that when living on a lower level of proteid consumption bodily strength and endurance are unquestionably increased; muscular fatigue and soreness as concomitants of severe or prolonged muscular effort diminish or are wholly wanting; thus raising the suggestion that under true physiological conditions the muscles of the body are capable of more prolonged effort, and with greater freedom from disagreeable after-effects than when the system is charged with an excess of nitrogenous and other waste incidental to large intakes of proteid food. In other words, consumption of proteid food in closer harmony with the true needs of the body is accompanied by a smoother and more efficient working of the bodily machinery; less friction and better results follow a daily diet in which excess is avoided and the intake made to correspond more closely with physiological requirements.

Those who are skeptical of the real value of a relatively low intake of proteid food frequently acquiesce in the general statement that as a physiological experiment it may be quite true that equilibrium, physical vigor, efficiency, etc., can be maintained by a smaller amount of proteid food, but they are inclined to the view that in the long run more abundant supplies of nutriment will be demanded in harmony with the ordinary customs of mankind. This is a reasonable objection, and one that time only can answer. It is quite possible—though not very probable—that an experiment of several years' duration even may fail to show certain deleterious effects which eventually may manifest themselves, assuming that the body does actually need more proteid food than our experimental results imply. This may be a purely theoretical objection, but it is one that is deserving of some consideration, since it is unquestionably true that there are many factors in the broad subject of nutrition not yet fully understood, and there are many phases of proteid metabolism not wholly clear. So far as any experimental evidence is concerned, however, there is nothing, in the writer's opinion, that can be construed as giving weight to this objection. Neither are there any observations bearing on the customs or habits of peoples or communities that can be adduced in favor of possible danger to the individual from a continued intake of proteid food in harmony with our experimental data; certainly none that is not equally susceptible of plausible explanation on some other ground.

As has been stated in another place,[2] a daily intake of 60 grams, or two ounces, of proteid is quite sufficient to meet the needs of a man of 70 kilograms body-weight, and this without increasing unduly the amount of non-nitrogenous food. In fact, for a man of the above weight doing an ordinary amount of work, the total calorific value of

[2] "The Nutrition of Man," p. 272.

the daily food need not exceed 2,800 calories. As compared with the ordinary statements of the body's needs, this means a saving of one half in the amount of proteid food and about one fifth in the amount of non-nitrogenous food daily. That these smaller amounts of food are quite sufficient to meet the needs of the body is indicated by the condition of the subjects after many months of living at these lower levels. Especially noticeable, because at that time wholly unexpected, was the decided gain in bodily strength and endurance manifested by all the subjects of experiment. This gain was spoken of as gain in "total strength," but the element of endurance was incorporated therein, since the final product[3] was a compound of the dynamometer tests of individual muscles and the number of times the individual could pull up and push up his body on the parallel bars. The natural interpretation of the results obtained was that the increased muscular efficiency was a direct or indirect result of the lowered proteid metabolism of the body. In other words, it might be reasoned that the smaller consumption of proteid food was a nearer approach to normal conditions, and as a result there was manifested an increased muscular efficiency. However this may be, bodily strength and endurance were certainly increased, and the question naturally arises, will this improved state of the body continue for any length of time under such conditions of diet? In other words, may we expect to find an improved physical condition of the body in following habits of life which seemingly accord more closely with true physiological needs, avoiding that excess of food intake that the common practises of mankind sanction?

One of the first subjects experimented with by the writer was Horace Fletcher, who in 1903 spent several months in our laboratory[4] and was at the same time carefully tested by Dr. William G. Anderson, director of the Yale Gymnasium, as to his physical condition. For some five years Mr. Fletcher had practised a certain degree of abstinence in the taking of food with, as he believed, important economy, *i. e.*, great gain in bodily and mental vigor and with marked improvement in his general health. He found that under his new method of living he was possessed of a peculiar fitness for work and with freedom from the ordinary fatigue incidental to extra physical exertion. In the laboratory observations made at that time, it was found that he was not metabolizing more than fifty grams of proteid per day, while at the same time his body was essentially in a condition of nitrogen equilibrium. Dr. Anderson, as the result of his observations on Mr. Fletcher, concluded that, considering his age, he had never seen an individual able to work in the gymnasium with fewer noticeable bad results, since he was able to do the work of trained athletes and not

[3] "Physiological Economy in Nutrition," p. 259.

[4] See POPULAR SCIENCE MONTHLY, June, 1903, p. 127.

give marked evidences of over-exertion, although not in training. At the time these experiments were tried Mr. Fletcher weighed one hundred and fifty-seven and a half pounds, and was in his fifty-fourth year.

While, naturally, we have not been able to obtain daily records of the quantity of food taken by Mr. Fletcher during the past four years, observations made from time to time have confirmed his general statement that he lives essentially at this same low level of proteid metabolism. In June, 1907, Mr. Fletcher was again at New Haven for some weeks, thus giving us an opportunity to test his rate of nitrogen exchange and his physical condition. It was found that the amount of nitrogenous or proteid food consumed daily never exceeded sixty grams, and that his nitrogen metabolism averaged not far from seven grams per day. His body-weight was found to be one hundred and seventy-seven and a half pounds. We thus had an opportunity of testing the physical endurance of a man who has for at least nine years practised a degree of physiological economy in nutrition, which means a daily consumption of proteid food in amount less than one half that called for by the ordinary dietary standards. It would seem reasonable to suppose that if a low nitrogen intake is destined eventually to prove detrimental to the individual, some sign of such deleterious effect would manifest itself during this period of time. If, on the other hand, consumption of proteid food in harmony with the lower dietary standards which the writer is disposed to advocate on the basis of his experimental results, is beneficial to the individual, then one might expect to find a continuance of the same physical vigor noted in the earlier observations on Mr. Fletcher, in spite of the fact that at this date the subject was nearly fifty-nine years of age.

Through the kindness of Dr. Anderson, of the Yale Gymnasium, Mr. Fletcher was subjected to a variety of tests, the outcome of which is best presented in the words of Dr. Anderson, as given to the writer in a report made under date of June 28, 1907.

On June 11, 1907, Mr. Fletcher again visited the Yale Gymnasium and underwent a test on Professor Fisher's dynamometer. This device is made to test the endurance of the calf muscles. (Soleus and gastrocnemius.) The subject makes a dead lift of a prescribed weight as many times as possible. In order to select a definite weight the subject first ascertains his strength on the Kellogg mercurial dynamometer by one strong, steady contraction of the muscles named—and then he finds his endurance by lifting three fourths of this weight on the Fisher dynamometer as many times as possible at two- or three-second intervals. One leg only is used in the lift, and, as indicated, the right is usually chosen.

Mr. Fletcher's actual strength as indicated on the Kellogg machine was not quite 400 pounds, ascertained by three trials. In his endurance test on the Fisher machine he raised 300 pounds 350 times and then did not reach the limit of his power. Previous to this time Dr. Frank Born, the medical assistant at the gymnasium, had collected data from 18 Yale students, most of whom were trained athletes or gymnasts. The average record of these men was 87.4 lifts, the extremes being 33 and 175 lifts.

It will be noticed that Mr. Fletcher *doubled* the best record made previous to his feat and numerous subsequent tests have failed to increase the average

of Mr. Fletcher's competitors. Mr. Fletcher informs me that he has done no training nor has he taken any strenuous exercise since February, 1907. On two occasions only during the past year he reports to have done hard work in emergencies; once while following Major General Wood in the Philippines in climbing a volcanic mountain through a tropical jungle on an island near Mindanao for nine hours; and once wading through deep snow in the Himalayan Mountains, some three miles one day and seven miles the next day, in about as many hours. This last emergency experience came through being caught in a blizzard near Murree, in northern India, at 8,500 feet elevation, on the way to the Vale of Kashmir. These two trials represented climatic extremes and Mr. Fletcher states that neither the heat nor the cold gave him discomfort, a significant fact in estimating physical condition.

Before the trial on the Fisher ergograph, the subject's pulse was normal (about 75); afterwards it ran 120 beats to the minute. Five minutes later it had fallen to 112. No later reading was taken that day. The hands did not tremble more than usual under resting conditions, as Mr. Fletcher was able to hold in either hand immediately after the test a glass brimming over with water without spilling a drop. The face was flushed, perspiration moderate, heart action regular and control of the right foot and leg used in the test normal immediately following the feat. I consider this a remarkable showing for a man in his fifty-ninth year, 5 feet 6½ inches in height, weighing 177½ pounds and not in training.

In order to make a more thorough test of Mr. Fletcher's powers of endurance under varying degrees of physical strain he underwent on the 17th, 18th, 19th, 21st and 22d of June, 1907, the following:

1. Going up 32 steps of a spiral stairway at natural speed.
2. While in the lying position, raising the trunk to a vertical position a prescribed number of times and continuing as many more times, at will, as agreeable.
3. While standing with arms upraised to the full bending forward and downward, touching the floor with the fingers without bending the knees.
4. While holding two 25-pound iron dumb-bells, first flexing the elbows and then raising the weights to arm's length above the head.
5. A daily test on the Fisher dynamometer, not for endurance, but for measuring pulse acceleration.

It became necessary to make a change in the character of the movements on the final day of the test on account of the chafed condition of the subject's skin, and we added:

6. "Running in place," with knee lifting forward and upwards to the extreme possible height.
7. Rapid extension of the arms upward, outward and downward.
8. Same as 7, but holding one-pound wooden bells in each hand.

Pulse readings were taken before and after each test, and in the following report the average pulse for each exercise is given:

After *quickly* climbing 32 spiral steps, five trials, the average pulse was 115.2 beats to the minute.

After trunk raising, five trials, 50, 60, 70, 100 and 100 times; the latter two trials in one day, five hours apart; average pulse, 115.2 beats.

After trunk bending, five trials, 60, 100, 150, 200 and 300 times; the latter two trials in one day, five hours apart; average pulse, 150 beats.

After lifting the 25-pound bells, five trials, 5, 5, 10, 10 and 10 times; average pulse, 138 beats.

After tests on the Fisher dynamometer, four trials, 50, 60, 60 and 60 times; average pulse, 120 beats.

After rapid arm work for three minutes, average pulse, 156 beats.

After similar work holding wooden bells (two minutes), average pulse, 156 beats.

After running in place as rapidly and as strenuously as possible for one minute, average pulse, 144 beats.

After each test the respiration and heart action, while active, were healthy, and, under such conditions, normal.

There was not the slightest evidence of soreness, stiffness or muscular fatigue either during or after the six days of the trials. The chafing of the skin was due to the rubbing of the "tights" worn while lying down and raising the trunk. Mr. Fletcher made no apparent effort to conceal any evidences of strain or overwork and did not show any. He informs me that he felt no distress whatever at any time.

During the thirty-five years of my own experience in physical training and teaching I have never tested a man who equalled this record. The latter tests, given in June, 1907, were more taxing than those given in 1903, but Mr. Fletcher underwent the trials with more apparent ease than he did four years ago. What seems to me to be the most remarkable feature of Mr. Fletcher's tests is that a man nearing sixty years of age should show progressive improvement of muscular quality merely as the result of dietetic care and with no systematic physical training.

Such a record of endurance as this, especially when made by a man fifty-nine years of age, can hardly fail to attract our attention. Further, when it is remembered that the subject of this test was not in training, the question naturally arises as to the cause of this phenomenal showing. Why a man of fifty-nine years of age, without training, should be able to far surpass the record for endurance made by young and vigorous athletes can only be surmised, but it certainly seems plausible to assume that the explanation is to be found in the careful dietary habits which this man has followed for the past nine years. In any event, it is fair to suppose that habits of life, leading to a relatively small intake of nitrogenous food, are not inimical to a general condition of physical efficiency and muscular endurance. We may go even farther and assume that the remarkable showing made by this subject is due directly to his temperate dietary habits. Mr. Fletcher would doubtless lay special stress upon his habit of thorough mastication and of abstaining from eating until the appetite strongly demanded food. This phase of the subject we need not discuss here. The main point is that this particular subject has during these nine years made a practise of consuming daily a quantity of proteid food not more than one half that demanded by ordinary dietary standards. In other words, his habits of living have been essentially in accord with the conclusions arrived at by our experimental studies bearing on the requirements of the body for proteid food.

We see in these results possible progressive muscular recuperation after middle life by means of diet alone. If a man by careful attention to his diet can show progressive gain in endurance and general efficiency after fifty without systematic training, it is a fact well worth knowing. In any event, the data afforded by this particular subject corroborate in striking fashion the conclusions arrived at by laboratory experimentation, and tend to confirm the view that there is perfect safety and probable gain to the body in a system of dietetics which approximates to true physiological requirements and avoids undue excess.

JEAN LOUIS RUDOLPHE AGASSIZ[1]

By Professor EDWARD S. MORSE
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JEAN LOUIS RUDOLPHE AGASSIZ was born in Motier, Switzerland, May 28, 1807, and died in Cambridge, December 14, 1873.

He was one of the great naturalists of the world, a student of Cuvier, beloved by Humboldt, counting every distinguished name in science as an admirer and idolized by his associates. At the age of twenty-four he had an international reputation. He had conferred upon him many degrees, one of which was the doctor's degree of medicine and surgery, in the preparation for which Von Siebold says he prepared seventy-four theses on anatomical, pathological, surgical and obstetrical subjects, also investigations in materia medica, medicina forensis and the relation of botany to these topics.

He studied at the medical school at Zurich, the University of Heidelberg and at the University of Munich. Investigations of the widest diversity in natural science were embodied in 415 papers, memoirs and books, many in quarto and folio, representing nearly ten thousand pages and a thousand plates.

Besides his profound attainments as a naturalist he was equally remarkable as a teacher and most eloquent as a lecturer. Always enthusiastic in his own work, he had the further power of inspiring this enthusiasm in others. At the age of twenty-three, in a letter to his brother, he said: "What troubles me is that the thing I most desire seems to me, at least for the present, farthest from my reach, namely, the direction of a great museum." He little foresaw that thirty-one years from that time he would see the inauguration with pomp and circumstance of the great museum at Cambridge of which he was the originator and director. Nor could he have anticipated that his son, profiting by his engineering and geological studies in the Lawrence Scientific School with which this museum was affiliated, should use that knowledge in securing the fortune by which the museum has expanded far beyond the most ardent imagination of its founder.

In the very prime of his manhood, in the very height of his fame,

[1] Read at the unveiling of the Agassiz tablet at the Hall of Fame, New York, May 30, 1907. In the preparation of this brief address I am indebted to Mrs. Elizabeth Agassiz's charming tribute to her husband in her "Life and Letters of Louis Agassiz" and to Marcou's "Life of Agassiz."

he came to our country, and by his enthusiasm, his eloquence, his winning and democratic ways, he won the hearts of all, and from his advent here may be dated the wide-spread love of natural science among the masses.

Agassiz's contributions as a naturalist covered the entire range of the animal kingdom. A study of his bibliography exhibits communications, papers and memoirs on every Cuvierian class. A further study of this bibliography indicates that, as a young man, he grappled with some of the most difficult groups of the animal kingdom. The fishes had been one of the most distracting divisions of the higher animals. The limitations of their genera, the homologies of their bony structure, had daunted most zoologists who confined their work to the description of species. Agassiz's first important work was the "Fishes of Brazil," based upon material brought home by Martius and Spix. This was done at the age of twenty-two. The work was written in Latin, dedicated to Cuvier, and illustrated with a folio of ninety plates. At the age of twenty-three he issued his prospectus of the natural history of the fresh-water fishes of central Europe, which was completed twelve years after, accompanied by a folio atlas of forty-one colored plates.

Difficult as was this task, he wrestled with a still more difficult one, namely, the "Fossil Fishes," and in nine years had completed this remarkable work in five quarto volumes with 400 colored folio plates. This publication alone placed him in the front rank of naturalists. An eminent geologist has written in regard to this work that Agassiz's power of classifying fossils and his success in reducing to order thousands of specimens of fishes, a great many of which were perfect puzzles to every one, was simply marvelous. The echinoderms, with their complicated covering of curious plates, spines and minute appendages, formed another most difficult group for study. From the number of fossil species in the rocks in his neighborhood Agassiz was led to a minute examination of both living and fossil forms which culminated in his great monograph of echinoderms with many plates.

The prodigious extent and character of the work done before he was thirty years old may be appreciated when it is stated that on a meager salary of $400 a year he established a lithographic press at Neuchatel, he employed two skilful artists, published a number of parts of his monograph of the echinoderms, several parts of his fossil fishes, made a profound study of the glacial phenomena of the Alps as well as of the geology and paleontology of the Jura and superadded to all this work the monographing of two molluscan genera, *Mya* and *Trigonia*. Ernest Favre, in his biographical notice of Agassiz, says, in regard to this period of his life, that he displayed an incredible energy, of which the history of science offers, perhaps, no other example.

His original way of dealing with subjects is well illustrated in his studies of fossil bivalve mollusca. It had been customary to describe the external markings of the shell and when possible the muscular impressions within. Agassiz soon realized the importance of studying the interior contour of the shell, and forthwith proceeded, by means of casts, to bring to light the relations of these fossils with their living representatives. His maxim was to have abundant material—thousands of specimens, if necessary—for any proper research. In studying glaciers he literally rode on the back of one for weeks at a time. He furthermore urged his students to read all that had been written on a subject before publishing.

Agassiz not only defined many new species of animals in various classes, but he was continually dwelling on the affinities and homologies among the various groups; more particularly their classification and their geographical and stratigraphical distribution. His studies in embryology and his familiarity with the work of Von Baer led him to recognize the general truth that the young of higher animals in their respective groups resembled the mature forms of animals lower down in the scale. From these studies he soon grasped the greater conception that this principle was carried out in time as well, and that fossil animals in the early horizons of geological history resembled the embryonic or early condition of higher animals now living and hence the idea of comprehensive or prophetic types. This same broad grasp of fundamental principles was remarkably illustrated in his studies of glacial phenomena in the Alps. One of his biographers says, "With his power of quick perception, his unmatched memory, his perspicacity, and acuteness, his way of classifying, judging and marshaling facts, Agassiz promptly learned the whole mass of irresistible arguments collected patiently during seven years by Charpentier and Venetz, and with his insatiable appetite and that faculty of assimilation which he possessed in such a wonderful degree he digested the whole doctrine of the glaciers in a few weeks," and added a great many new and important facts.

From his study of the glaciers of the Alps he soon announced his belief that the whole northern hemisphere had at one time been covered by an ice sheet. The various records of this vast sea of ice which had been interpreted by the most eminent geologists as the result of diluvial action and flowing mud he rightly attributed to the action of ice. In the face of the most strenuous and even bitter opposition he triumphantly established the former existence of the Great Ice Age. Subsequent studies, while modifying the limitation of the great ice sheet, have only strengthened the views of Agassiz.

Agassiz's absorbing interest in the structural relations of animals led him to define with greater accuracy the limitation of various groups.

As a student of the great French naturalist, Cuvier, he became an eloquent advocate of the existence in nature of four great branches of the animal kingdom. He was early convinced that branches, classes, orders, families and genera had as distinct an existence in nature as species, and his life work was to make clear and rigid their definition. His eager desire to understand the relations existing between obscure forms was expressed one day in a private talk to his pupils, when he earnestly exclaimed, "The lamprey eel has been my puzzle and my misery for twenty years."

Not only in many technical essays, but as an eloquent teacher, he made these principles of classification so plain that vast audiences were able to grasp his conceptions. Those who heard his lectures on the subject will never forget the vivid way in which he impressed upon his auditors these views emphasized by graphic blackboard drawings.

In his methods of study in Natural History he presented in a popular form the leading features of his belief in the systematic relations of animals as embodied in his famous "Essay on Classification." The following quotation from his Methods of Study will indicate the ideas which were surely preparing the ground for the acceptance of the theory of evolution:

> Man is the crowning work of God on earth, but though so nobly endowed, we must not forget that we are the lofty children of a race whose lowest forms lie prostrate within the water, having no higher aspirations than the desire for food; and we can not understand the possible degradation and moral wretchedness of Man, without knowing that his physical nature is rooted in all the material characteristics that belong to his type and link him even with the fish. The moral and intellectual gifts that distinguish him from them are his to use or to abuse; he may, if he will, abjure his better nature and be Vertebrate more than Man. He may sink as low as the lowest of his type, or he may rise to a spiritual height that will make that which distinguishes him from the rest far more the controlling element of his being than that which unites him with them.

Not only by such expressions just quoted, but in other statements, he certainly prepared the way for the more prompt recognition of Darwin's views.

Inspired by the belief in the existence in nature of categories of structure, he strengthened old homologies and established many new ones. In representing the four Cuvierian branches by schematic lines, he did not draw a series of lines one above the other, or enclose each group by sharply defined brackets, but drew these lines, parallel it is true, but side by side in an ascending scale, slightly overlapping. He endeavored to indicate by such a diagram his belief, which was correct, that the higher members of a lower group were more advanced in structure than the lower members of a group next above. Thus while the vertebrates were higher as a branch than the articulates, the highest class of the articulates, the insects, were higher in structure than many

of the lowest vertebrates. In this way he broke up the idea that the animal kingdom formed a continuous ladder in creation, from the lowest form to man. This was an important approach to a phylogenetic diagram, for it was readily seen that the lower forms in each great division had closer affinities with each other than existed among the higher members. In other words, that his schematic lines should not be made parallel, but should converge below—a genealogical tree in fact. His generalized or prophetic types lend overwhelming support to this conclusion.

It has been repeatedly said, and with truth, that Agassiz's teachings paved the way for the prompt acceptance of the theory of evolution—first, because he familiarized the great public with a structural knowledge of the animal kingdom and the affinities existing between the different groups, and, second, because he demonstrated the recapitulation theory of Von Baer, and added the great conception that the history of the animal kingdom from the earliest geological horizons added further proof of these principles. Agassiz came to an environment well fitted to encourage him. He came to an intellectual center famous for its leadership in science and letters, but the hearty reception accorded him in widely separated regions leads to the conviction that had he settled anywhere in the Country he would have inspired the same enthusiasm and induced hard-headed legislators everywhere to have voted large appropriations, and private citizens to contribute generous sums. It required only his touch to bring into recognition names among us that had before his magic influence been known only in limited circles. Men of the caliber of those of 1846 are a thousand times more widely known to-day, not because of the changed character of the public press, which celebrates with equal prominence and impartiality girl graduates of a public school and men who have revolutionized the world by their inventions, but becouse he made us appreciate the worth of an investigator. Our nation has always believed in education and public schools, and hence has universally approved of high endowments for educational purposes. His great plea and one that had its effect on the legislators was that the museum was an educational institution, that it was to be opened every day free to the public and that it was a sound investment, though its dividends were wholly intellectual. A few personal reminiscences may be of interest at this point. In the early part of the civil war, one of our class enlisted and received an appointment as an officer of the line—the rest of us bought a fine sword and presented it to him. On showing the sword to Agassiz, he instantly threw himself into the attitude of a fencer and became absorbed in thrust and parry, utterly unconscious of our amazement at his earnestness and skill. We learned afterwards that as a student at Munich he had not only fought a number of student duels

in which he was always the victor, but on one occasion he had challenged a whole class, whereupon the best swordsman was selected to meet him, when he insisted that he had really challenged every member of the class to fight. After four had crossed swords with him and been vanquished the remainder were quite ready to retire. Agassiz with all his genius had no capacity for business and, as he admitted, was incapable of doing a simple sum in addition; nevertheless, he plunged into investigations which to carry out involved the expenditure of large sums of money. Mrs. Agassiz in the charming tribute to her distinguished husband says:

> He was frugal in his personal habits. At this very time, when he was keeping two or three artists on his slender means, he made his own breakfast in his room and dined for a few cents a day at the cheapest eating houses. But where science was concerned the only economy recognized, either in youth or old age, was that of an expenditure as bold as it was carefully considered.

While expressing his great appreciation of the many honors given him by distinguished societies, he seemed to be indifferent to the certificates of these honors. As an illustration of this indifference I may cite an experience that a few of us had with an enormous mass of pamphlets which were unpacked and which Agassiz asked us to classify and arrange by their respective subjects. Intermixed with these pamphlets were numerous diplomas, some of them badly wrinkled, attesting to his election as associate or honorary member of great societies and academies, university degrees, and, if I remember rightly, medals of honor also.

Very few are aware of the profound influence Agassiz's devotion to his work and his enthusiasm had on the character of Harvard College. To apply an expression of Froude, he came in to this staid college community like a meteor out of the clear sky. One day as he crossed the college campus I drew a sketch of him: it contradicts every custom and tradition of the Harvard professor since the foundation of the college in 1638. On his head a soft hat, in his pockets his hands, in his mouth a cigar! President Eliot, in his address at the Agassiz commemorative meeting of the Cambridge Historical Society, said that Agassiz's ability in securing from hard-fisted members of the General Court large appropriations for his museum, excited the envy of other departmental chiefs. Yet in obtaining these large sums from the legislature, and from private citizens as well, he finally provoked the habit of liberal giving to the college as a whole. Thus the college grew into a university, and the inception of this growth dates from the advent of Agassiz. His advice was followed in shaping the work of the Smithsonian Institution. A similar influence must be accredited to him in enlarging the work of the United States Coast and Geodetic Survey. Professor Bache, then superintendent, was an intimate friend

of Agassiz, and the broadening views of Agassiz on the work of this important branch of the national government was marked. The American Association for the Advancement of Science is indebted to Agassiz for the remodeling of the old Society of Geologists and Naturalists along the line of the British Association, of which he had long been a member. He became president of the association in 1851. Agassiz, Bache and Henry were the leading spirits in originating the National Academy of Sciences. The character of the man is indicated by the fact that the highest authorities in art, science and literature were immediately drawn to him and found in him a true friend and a charming companion.

The students associated with Agassiz at the dedication of the museum in Cambridge with few exceptions became heads of many of the great museums of the country.

Professor Hyatt was, at the time of his death, custodian of the Boston Society of Natural History. Dr. Scudder had preceded him in the same office. Professor Shaler continued at Harvard as professor of geology and became dean of the Lawrence Scientific School. Professor Putnam, one of the originators of the Peabody Academy of Science in Salem, and for years director of its museum, is now director of the Peabody Museum at Cambridge. Professor Verrill has been professor of zoology at Yale since his graduation and is director of the museum at New Haven. Professor Packard, for some years director of the Peabody Museum at Salem, was at the time of his death, professor of zoology at Brown University. Professor Bickmore was closely identified with the inception of the American Museum of Natural History in New York, was its first director and continued in the office for many years, and the writer has for twenty-seven years been director of the Peabody Museum at Salem.

This record is certainly a credit to the great teacher whose pupils adhered to the initial impulse imparted to them by their master.

At the age of twenty-two, in a letter to his father, he wrote:

> I wish it may be said of Louis Agassiz that he was the first naturalist of his time, a good citizen, and a good son, beloved by those that knew him. I feel within myself the strength of a whole generation to work toward this end, and I will reach it if the means are not wanting.

This boyish prophecy was fully established as attested by the glorious records of his life.

NOTE TO THE EDITOR: In view of the distracting state of zoological nomenclature at the present time with the habit of regarding the slightest deviation in structure as of generic value with the result that nearly every species has a separate generic name, it may be regarded as a misfortune that Agassiz could not have established on a sure and enduring foundation his various categories of classification. In a conventional manner it would be profitable to adopt his definitions, even if the groups have no real existence in nature. Only in this

way can relief be secured from a condition which is confusing and exasperating.

As an illustration of Agassiz's firm adherence to his principles of classification so clearly elaborated in his famous essay on the subject, I may be excused for giving a letter written to me a few days after I had explained to him my views regarding the systematic position of the Braciopoda:

Your statements of last Saturday haunt me and I can not rest before I have seen more of your facts concerning the Anneliden affinities of the brachiopods. The most telling evidence *in your favor*[2] you have never yet alluded to, at least not in my presence. But I must be cautious and wait till I see and hear more of your facts. When and where can I see you again? This is not a question of structural complication.

Very truly yours,

L. AGASSIZ.

CAMBRIDGE, Jan. 2, 1871.

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[2] The italics are his.

THE ORIGIN OF SLAVERY AMONG ANTS

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THE researches of the past few years have materially changed our views on the significance and phylogenetic origin of the so-called slave-making instincts among ants. And although the subject still involves many unsolved problems, we are now in a position to look back on its history and marvel at our too implicit confidence in certain analogies, at our neglect of the basic principles of phylogenetics, and at the inept questions we so long persisted in asking.

Slavery, or dulosis, is a rare phenomenon among ants. In its pure form it is known to occur only in two of the several thousand described species, namely, in the sanguinary or blood-red slave-maker (*Formica sanguinea*) and the amazon (*Polyergus rufescens*). These species, with their various subspecies and varieties, are peculiar to the north temperate portions of Europe, Asia and America. The phenomenon was first discovered by J. Pierre Huber (1810)[1] and most completely described by him and by Forel (1874)[2] These investigators, of course, fixed their attention on the behavior of the workers. To this aspect of the subject later writers have added little of importance, and have merely fallen into a natural error of continuing in the same path as their illustrious predecessors. This was the case, for example, with Darwin[3] and with Wasmann, who for the past quarter of a century has been observing the slave-making ants of Europe. Huber and Forel showed that the workers of *F. sanguinea* and *P. rufescens* make periodical forays on colonies of ants belonging to the *F. fusca* group, carry home the worker cocoons and larvæ, and permit some of these to hatch and to survive with them in the same formicary. An eminently predatory species thus comes to live in intimate symbiosis with workers of an alien species which are said to function as slaves, or auxiliaries. *F. sanguinea* is a powerful and very plastic species which continues to exercise all the fundamental ant instincts in the presence of its slaves. It can excavate galleries in the soil, obtain its own food and bring up its own young. *Polyergus,* however, is abjectly dependent on its auxiliaries. It is no longer able to excavate a nest, care for its own

[1] "Récherches sur les mœurs des fourmis indigènes," Paris et Genève, 1810.

[2] "Les Fourmis de la Suisse," Zürich, 1874.

[3] "On the Origin of Species by Means of Natural Selection," third edition, London, John Murray, 1861, p. 244.

offspring, or even to take food, except from the tongues of the alien workers. It is therefore properly considered as representing a more advanced stage of parasitism than *sanguinea*. A few species belonging to the Myrmicine genera *Tomognathus* and *Strongylognathus* seem to possess analogous instincts, but too little is known of their habits to enable us to make very definite statements concerning them.

It was, of course, impossible to do more than speculate on the phylogeny of the slave-making instincts of *sanguinea* and *Polyergus* without a knowledge of the ontogenetic source and development of these instincts, and as these are social activities, that is, carried out simultaneously by a number of cooperating organisms, it was necessary to learn something about the origin and development of the ant colony as a unit. The bearing on the origin of slavery of the obvious and fundamental fact that there is a double ontogeny and phylogeny in social organisms, namely, one of the colony as well as one of the individual, has been appreciated only within the past few years and has completely changed the aspect of the subject.

In the great majority of ant species the colony arises and develops in the following manner: The single female, or queen, after mating during her marriage flight, descends to the earth, divests herself of her wings, digs a small cell in the soil, or enters some preformed cavity under a stone or in the tissues of a plant, lays a number of eggs, feeds the resulting larvæ with a salivary secretion, and guards and nurses them till they mature and constitute a brood of diminutive workers. These now proceed to enlarge the nest, to forage for food, both for themselves and their mother, and to care for the succeeding broods of young. The queen thenceforth gives herself up exclusively to feeding from the tongues of her offspring and to laying eggs. The colony grows apace, the workers increasing in number, size and polymorphism with successive broods. Eventually males and virgin queens are produced, though often only after the expiration of several years, when the colony may be said to have completed its ontogenetic development.

It will be seen from the foregoing description that the mother queen lapses from the position of an independent organism with remarkable initiative to that of a parasite dependent on her own offspring. The latter stage in her life is of much longer duration than the former. This singular ontogenetic change in the instincts of the queen should be noted, as it foreshadows an important phylogenetic development exhibiting two different modifications, one of which is excessive, the other defective, in comparison with the primitive and independent type of colony formation. The excessive, or redundant, type is known to occur only among the Attiine ants of tropical America. These raise fungi for food and are quite unable to subsist on any other substances. The queens are often very large, especially in the typical

genus *Atta,* and not only manage to bring to maturity a brood of workers, but at the same time, as von Ihering,[4] Goeldi[5] and Jacob Huber[6] have shown, have energy to spare to devote to the cultivation of a fungus garden. With the appearance of the first brood of workers, however, these queens, like those of most ants, degenerate into parasites on their own progeny.

This dependent stage, which, as I have said, is of much greater duration than the independent stage in the long life of the queen, leads to a number of phylogenetic developments of the defective type. These developments first manifest themselves in the adoption of young queens by adult workers of their own species. A word of explanation will make this clear. In the colonies of many species of Formicidæ we find several queens—in fact, there are comparatively few ants whose adult colonies do not contain more than one of these fertile individuals. And a study of the growth of such colonies shows that the supernumerary queens are either daughters of the original single queen that founded the colony, or have been adopted from other colonies of the same species. Hence these queens are either virgins, or have been impregnated by their own brothers (adelphogamy of Forel) in the parental nest, or have been captured by the workers and carried into the nest after descending from their nuptial flight. This forcible adoption leads necessarily to a complete suppression of the independent stage in the life of such queens. I have shown, in another article, that merely removing a queen ant's wings with tweezers will at once call forth the dependent series of instincts, and the same result is undoubtedly produced when the workers deälate the virgin or just-fertilized queens of their own or other formicaries. Such queens, finding themselves surrounded by a number of accomplished nurses, the workers, proceed at once to act like old queens that have already established their colonies and brought up a brood.

From this condition of facultative adoption to an obligatory adoption of the queen by workers of her own species is but a step. And here there are three possibilities: first, the queen can establish a colony only with the aid of workers of her own species and of the same colony. This condition seems not to obtain among ants, although it is well known in the honey-bees. Second, the queen must either be adopted by the workers of her own species of the same or another colony, or

[4] "Die Anlage neuer Kolonien und Pilzgärten bei Atta sexdens," *Zool. Anz.,* XXI., 1898, pp. 238–245, 1 fig.

[5] "Beobachtungen über die erste Anlage einer neuen Kolonie von Atta cephalotes," *C. R. 6me Congr. Internat. Zool.,* Berne, 1905, pp. 457–458, and "Myrmecologische Mitteilung das Wachsen des Pilzgärtens bei Atta cephalotes betreffend," *ibid.,* pp. 508–509.

[6] "Ueber die Koloniengründung bei Atta sexdens," *Biol. Centralbl.,* XXV., 1905, pp. 606–619, 625–635, 26 figs.

by workers of an alien species. This is the case with many queen ants that have lost the power of establishing colonies unaided. Third, the queen must always be adopted by an alien species. This is the case in certain ants, especially in the highly parasitic forms that have lost their worker caste. The three conditions here enumerated clearly represent the transition from parasitism of the queen on the same, to parasitism on an alien species. The latter alone is commonly regarded as true parasitism, but the former, which, of course, can occur only among social organisms or during social stages in the lives of solitary organisms, is parasitism in every essential particular. It is not confined to ants and other social insects, but has analogies also in human societies (trusts, "grafters," criminal and ecclesiastical organizations) and in human families (when the parents become senile).

Ant colonies are such closed and exclusive societies that the adoption of strange queens, even of the same species but from alien colonies, usually meets with insuperable opposition on the part of the workers, and, as a rule, female ants have to overcome even greater hostility when they seek adoption in colonies of alien species. There are, nevertheless, at least three different methods of overcoming this hostility and of effecting an adoption. These may be taken to characterize three different forms of social parasitism, as follows:

1. *Temporary Social Parasitism.*—I have given this name to a form of parasitism which I first observed in our American *Formica difficilis* var. *consocians.*[1] The fertilized female of this ant, quite unable to found a colony unaided, enters a colony of *F. schaufussi* var. *incerta* and is adopted with surprising facility. The queen of the latter species disappears, in some manner hitherto unknown, and the *consocians* brood is reared by the *incerta* workers, which, after functioning as nurses, gradually die off and leave a pure *consocians* colony thenceforth able to wax large and lead an independent and aggressive existence. This interesting type of parasitism occurs in most, if not all, *Formicæ* of the *exsecta* and *rufa* groups, both in America and in Europe, in a Myrmicine ant, *Aphænogaster tennesseensis* (parasitic on *A. fulva*) and in a Dolichoderine ant, *Bothriomyrmex meridionalis* (parasitic on *Tapinoma erraticum*). The females of these parasitic species tend to become greatly reduced in size (*F. difficilis* and several allied species: *F. dakotensis, microgyna, impexa, nepticula, suecica,* etc.) or at any rate to become smaller than the queens of their host species (*F. truncicola, wasmanni, oreas, ciliata, crinita, pressilabris,* etc.). This is clearly an adaptation to a mode of life for which an endowment of fat and vigorous muscle is not needed, since these various queens do not have to starve for weeks or even months while bringing up a brood

[1] "A New Type of Social Parasitism Among Ants," *Bull. Am. Mus. Nat. Hist.*, XX., 1904, pp. 347–375.

of workers, as in the case of most ants. Santschi has recently made the illuminating discovery that the queen *Bothriomyrmex,* after entering the nest of *Tapinoma,* actually decapitates the queen of the host species and is adopted in her stead. In the other cases the disappearance of the host queen has not been accounted for. In the case of *F. incerta* it is conceivable that she may be ejected from the colony or be killed by her own workers as in the colonies of the Algerian *Monomorium salamonis* infested with *Wheeleriella,* a case to be considered presently. For the *consocians* type of social parasitism Santschi[8] has suggested the name "tutelary" parasitism, because the young of this species are reared by workers older than the parasitic queen.

2. *Slavery, or Dulosis.*—In this case, as I have shown for the American *F. sanguinea,*[9] the female enters a *Formica* colony belonging to some variety of the *F. fusca* or *schaufussi* group, kills or puts to flight the workers that attack her and hastily appropriates a number of worker larvæ or cocoons. These she carefully guards till they hatch, when she is surrounded by a loyal brood—of an alien species, to be sure, but nevertheless both able and inclined to bring up her brood when it appears. This is "pupillary" parasitism, to use Santschi's term, since the nurses, or host ants, are younger than the parasitic queen. In this case the queen of the host species is probably put to flight at the time the *sanguinea* queen enters the nest. *Polyergus rufescens* colonies are, perhaps, founded in the same manner, but unequivocal observations on the queens of this species are still lacking. Not only is slavery, at least as manifested in *sanguinea,* distinguished from the other forms of social parasitism by the aggressive behavior of the queen, but also by a peculiarity of her own workers. These inherit from their mother the instinct to enter nests of the host species, and appropriate the young, but these queen instincts are intimately associated with the feeding instincts of the workers, as the latter forage in companies like so many nondulotic ants and consume many of the captured pupæ. Hence the futility of all attempts, like those of Darwin and Wasmann, to understand slavery from a study of the behavior of the workers alone.

Wasmann[10] and Santschi believe that slavery has arisen from temporary parasitism, but although I myself first advanced this opinion, I have been compelled to abandon it. Wasmann found that a colony of *Formica truncicola,* which he has shown to be a temporary social para-

[8] "A Propos des Mœurs Parasitiques Temporaires des Fourmis du Genre Bothriomyrmex," *Ann. Soc. Entom. France,* 1906, pp. 363–392.

[9] "On the Founding of Colonies by Queen Ants, with Special Reference to the Parasitic and Slave-making Species," *Bull. Am. Mus. Nat. Hist.,* XXII., 1906, pp. 33–105, pls. VIII.–XIV.

[10] "Ursprung und Entwickelung der Sklaverei bei den Ameisen," *Biol. Centralbl.,* XXV., 1905, pp. 117–127, 129–144, 161–169, 193–216, 256–270, 273–292.

site in all essential respects like *F. consocians,* would accept and rear *fusca* pupæ placed in the nest. This, however, is not dulosis. In order to establish his case he would have to prove that the *truncicola* workers can also make periodical forays on *fusca* for the sake of capturing their young, and there is no more evidence that *truncicola* can do this than there is of similar behavior on the part of *consocians.* Santschi, if I understand him correctly, believes that the *sanguinea* colony restricts its forays to the scattered fragments of the original *fusca* colony from which the queen secured her first supply of auxiliaries, and that the slave-making expeditions cease when these fragments are exhausted. This assumption seems to explain the fact that old *sanguinea* colonies are sometimes slaveless and pure, like the adult colonies of *consocians, truncicola,* etc. It is, however, rendered highly improbable by the fact that both in Europe and in North America *sanguinea* colonies not infrequently contain slaves of two or more different species or varieties. There is also some evidence that the same colony may have slaves of different species at different times. Professor Forel recently showed me near Morges, Switzerland, a colony of *Polyergus* which in 1904 contained only *F. rufibarbis,* but during the current year contained only *F. glebaria.* The similarity between old *sanguinea* colonies and adult colonies of temporary parasites like *F. consocians,* is more probably the result of two very different processes: in the former case of a languishing or lapsing of the slave-making instincts with age, in the latter, as I have shown, of a gradual extinction of the tutelary workers.

3. *Permanent Social Parasitism.*—This occurs in the following rare and monotypic Myrmicine ants: the European *Anergates atratulus,* parasitic on *Tetramorium cæspitum,* the Tunisian *Wheeleriella santschii,* parasitic on *Monomorium salomonis,* the North American *Epœcus pergandei* (on *Monomorium minutum* var. *minimum*), *Sympheidole elecebra* (on *Pheidole ceres*) and *Epipheidole inquilina* (on *Ph. pilifera coloradensis*). All these parasites are unique among ants in lacking a worker caste, so that they are compelled to live permanently with their respective host species. Santschi[11] has recently shown that the just-fecundated queen of *Wheeleriella* enters a *Monomorium* colony and is adopted by the workers, which then actually proceed to kill their own queen. The same conditions probably obtain also in the other cases, as the parasitic queens are too feeble to assassinate the host queen after the manner of *Bothriomyrmex.* In *Anergates* the degeneration of the species as a result of permanent parasitism is extreme: the male is reduced to an apterous, pale and anæmic, sluggish, pupa-like creature which mates in the maternal nest with its own sisters (adelphogamy),

[11] Forel, "Mœurs des Fourmis Parasites des Genres Wheeleria et Bothriomyrmex," *Rev. Suisse Zool.,* XIV., 1906, pp. 51–69; "Nova Speco Kaj Nova Gentonomo de Formikoj," *Internacia Scienca Revus,* 4° Jars, 1907, pp. 144, 145.

as Forel has shown, and as I was able to observe during the past June in a large *Anergates-Tetramorium* colony at Vaux, Switzerland. This colony contained upwards of 1,000 winged female *Anergates* and several hundred males. Many of the former, placed on the craters of strange *Tetramorium* nests, entered these at once. The *Tetramorium* workers never killed these females, though they often seized them, carried them some distance from the nest and cast them away. The males, too, were not killed, although they were more forcibly and immediately ejected. This behavior is very suggestive, for *Tetramorium* workers when placed on the craters of strange colonies of their own species are at once pounced upon and killed.

It is not improbable that all three of these derivative types, namely, temporary, permanent and dulotic parasitism, have developed independently out of the primitive adoptive type of colony formation, although the details of this development are still very obscure. I have already given my reasons for believing that slavery did not arise directly from temporary parasitism. Owing to the excessive specialization of the permanent parasites and the loss of the worker caste among these species, it is not so easy to determine whether they have arisen from temporarily parasitic or from dulotic species, for it is conceivable that they may have arisen from either, especially as there are other ants, such as *Strongylognathus* and *Tomognathus,* which combine peculiarities of both of these categories. The species of *Strongylognathus* are peculiar to the palearctic fauna and, like *Anergates,* live with colonies of the extremely abundant and ubiquitous *Tetramorium cæspitum.* The workers and females have sickle-shaped mandibles like *Polyergus.* Two species, *S. rehbinderi* and *S. huberi,* as Forel has shown, still possess vestiges of slave-making instincts. In *S. testaceus,* however, which is the common European form, the workers are greatly reduced in number, showing, as Forel has suggested, that this caste is on the eve of disappearing completely and thus leading to conditions like those of *Anergates* and the other permanent parasites. Wasmann once found a *S. testaceus-Tetramorium* colony containing fertile females of both species, and during the past June Professor Forel and I found a similar colony on the Petit Salève, near Geneva. This colony contained a fertile *Tetramorium* queen. The much smaller *Strongylognathus* queen could not be found, but must have been present, as there were young pupæ of this species in the nest. It is evident in this case, therefore, that the parasitic and host queens manage to live side by side (allometrobiosis of Forel). This condition arose, perhaps, from slavery or temporary parasitism by a suppression on the part of the *Strongylognathus* queen of the instinct to kill or drive away the *Tetramorium* queen.

The genus *Tomognathus* is represented in northern Europe by *T.*

sublævis (parasitic on *Leptothorax acervorum*) and in North America by *T. americanus* (parasitic on *L. curvispinosus*). The former was supposed by Adlerz[12] to have only ergatoid, or worker-like females, but Viehmeyer[13] has recently found winged females as well, and I had previously shown that such individuals exist in our American form. The workers of both species resemble those of *Polyergus* and *Strongylognathus* in having blunted or obsolete domestic instincts. Adlerz's observations seem to indicate that the European *Tomognathus* may be dulotic, but they do not altogether preclude the possibility of permanent parasitism. As there are no observations on the behavior of the recently fecundated queens, it is impossible to decide whether the form of symbiosis exhibited by these ants arose from dulosis or from temporary parasitism or merely from a condition of xenobiosis like that of the North American *Leptothorax emersoni* or the European *Formicoxenus nitidulus*.[14]

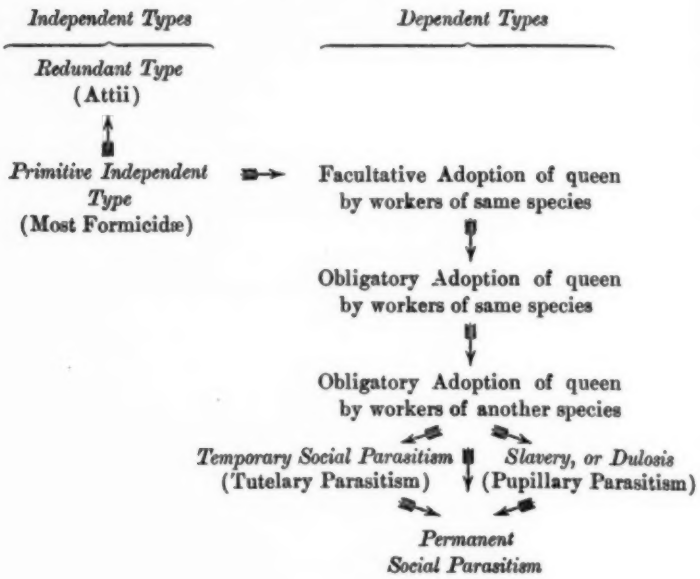
The accompanying diagram will serve to illustrate the phylogenetic relationships of the different types of colony formation among ants as formulated in the preceding paragraphs.

The foregoing discussion shows very clearly that a rational explanation of slavery among ants can be found only by recognizing the phenomenon as a form of parasitism. This conclusion is indeed forced upon us by a comparative study of the various allied forms of social symbiosis, of the ontogeny of the ant-colony, that is, of the way in which it is started and develops, and by a study of the instincts of the queen. We myrmecologists seem to have been hampered in reaching this conclusion by a knowledge of the habits of the queen honey-bee. This insect is peculiar in being permanently and exclusively in the adoptive

[12] "Myrmekologiska Studier—III., Tomognathus sublævis Mayr.," *Bih. Svensk. Vet. Akad. Handl.*, XXI., Afl. 4, 1896, 77 pp., 1 taf.

[13] "Beiträge zur Ameisenfauna des Königreiches Sachsen," *Abhandl. naturwiss. Gesell. Isis*, Dresden, 1906, Heft II., pp. 55–69, Taf. III.

[14] Since the manuscript of this article was sent to press I have received from my friend, Mr. H. Viehmeyer, of Dresden, an interesting communication, in which he describes his experiments with a number of naturally dealated and therefore presumably fecundated queens of *Tomognathus sublævis*, *Formica sanguinea*, *Polyergus rufescens* and *F. truncicola*. These queens were introduced into strange colonies belonging to the normal hosts of their respective species. The results obtained with *F. sanguinea* and *truncicola* fully confirmed my observations on the American *sanguinea* and *consocians*. The queens of the typical European *Polyergus rufescens* were much more passive than those of the American subspecies *lucidus*, used in my experiments, and were adopted on the second or third day by the slave species *F. rufibarbis*, but not by *F. fusca* till a much longer period had elapsed. An ergatoid *Tomognathus* queen placed in a colony of *Leptothorax acervorum* "presented the same picture as *sanguinea*. The *Leptothorax* fled with their larvæ and then attacked the queen. During the course of the day, however, the latter managed to kill all of the *Leptothorax*."



or dependent stage, that is, she is unable to found a colony or even to exist apart from workers of her own species. And as the queen ant passes most of her life in similar dependence on her workers, namely, after establishing her colony, the earlier and more characteristic manifestations of her instincts and her marvelous initiative and plasticity were either disregarded or deemed to be of little importance. Attention was concentrated on the worker slave-makers whose activities represent a combination of queen and worker instincts. Darwin was thus led to derive the slave-making from the foraging instincts, and Wasmann—well Wasmann could only keep repeating or implying that the slave-making ants made slaves, because they were endowed with a slave-making instinct—a fine modern example of Molière's famous opium fallacy and of the resources of scholastic methods in zoology! Wasmann supposed that *F. sanguinea* is possessed of an extraordinary fondness for educating the young of the alien *fusca*. This was quite incomprehensible, especially as *sanguinea* workers are in no respect degenerate or dependent on their auxiliaries. Since I have examined many colonies of the European *sanguinea*, which, as a rule, rears much fewer auxiliaries than our American forms of the same species, Wasmann's assumption seems to me to be preposterous. After the habits of our temporary parasites and especially after the behavior of the young *sanguinea* queens had been studied, the relations of the dulotic species to particular hosts were easily understood, for the young queens are reared by workers of a particular host species (*fusca* or *schaufussi* or some of their varieties) or at any rate meet them frequently in the parental

nest. What is more natural, therefore, than that the queens, when ready to establish their colonies, should seek out the nests of these same species? The *sanguinea* workers, too, are reared by auxiliaries of the same species, so that we are not surprised to find that it is against colonies of these that the dulotic expeditions are directed. The absence of any tendency on the part of the *sanguinea* to rear or adopt the males and females of the host species may be due merely to a lack of familiarity of the slave-makers with these sexual forms, which in all probability are characterized by a peculiar odor unlike that of the co-specific workers.

Thus is dissipated much of the mystery with which the subject of slavery has been invested, and the phenomenon becomes intelligible as a form of parasitism in which the slaves are really the host. The dulotic ants differ from the temporary and permanent parasites not only in the peculiarity of the worker instincts, but also as representing parasites with a synthetic host. In other words, the workers, when they snatch the larvæ and pupæ from different nests of one or more varieties of *F. fusca or schaufussi,* are actually constructing a unitary colony out of fragments of several colonies of the host species. This peculiarity, as I have shown, arises from the inheritance of female instincts by the workers and a fusion of these with the foraging instincts which the worker slave-makers share with this caste in many other Formicidæ. Santschi expresses a similar opinion when he says: "In fine, slavery reduces itself to a form of pupillary parasitism that perpetuates and extends itself beyond the confines of the nest." His distinction of tutelary and pupillary parasitism is useful, as it calls attention to a more active and a more passive form of this phenomenon, but the distinction should not be overworked. Although the tutelary form would seem to lead more readily to permanent social parasitism with all its attendant degenerative characters, we must remember that *Polyergus,* though very passive in the hands of its slaves, is extremely aggressive when plundering the nests of the host species, whereas species like *F. consocians* and *truncicola,* though very passive in the earliest stages of colony formation, are very aggressive as soon as their colonies have emancipated themselves from the host species. The pupillary and tutelary types are, moreover, already foreshadowed as consecutive ontogenetic stages in the behavior of most ant-queens, for the independent stage in colony formation is pupillary, whereas the closing years of the queen's life are passed in a condition of tutelary parasitism on her own offspring and species.

A TRIP AROUND ICELAND.

By L. P. GRATACAP
AMERICAN MUSEUM OF NATURAL HISTORY

III

REYKJAVIK was reached; the capital of Iceland, that first old landfall for the anxious vikings, who found that when they threw over their Lares and Penates those undiscerning deities floated ashore upon this inauspicious coast. The choice has a certain pictorial value, but for commercial purposes those old gods should have exercised more discretion, and commercial interests are beginning to weigh overpoweringly in this arctic metropolis. To the immediate north the snow-crowned Esja shines, to the southeast the sturdy eminences of the Lönguhlitharfiall swim upward over the horizon; and still farther south the volcanic peaks of Krisuvik, where the sulphur quarries are. Then to the northwest like a titanic gleaming gem Snaefells with its ice mantle draws to its overmastering beauty every eye. But this in clear weather, and clear weather is not a very plentiful article in Iceland. In bad weather, which is a trifle more common, the steamers may keep their imprisoned passengers for four days before they can land. The harbor is called so by a pleasant boreal fiction, which is not creditable to Icelandic hospitality. It is expected that next year an appropriation of some $400,000 will be granted permitting Mr. Smith, the official harbor surveyor of Norway, to execute his *accepted* plans for improving these inclement conditions.

The town of Reykjavik contains about ten thousand inhabitants. It has doubled its size in five years. Stores have developed, and the caravans from the interior can return home laden with the furnishings of a modern household, not omitting wall paintings and bath-tubs. It is scattered over a hilly surface with its more pretentious buildings displayed near the water front and around the square where the statue of Thorwaldsen faces the Althing (Parliament) house. The buildings are of wood (all brought from Denmark, Norway or Scotland), frequently sheathed with corrugated iron, with foundations, in many cases, of concrete blocks. Coals from Scotland are shipped here in great quantities, and the houses are thus provided with comfortable heating equipments. Some of the houses are also stuccoed. At times there is an architectural elaboration noted, but the houses are usually plain and serviceable. Two bank buildings of concrete blocks (the manufacture of these blocks is carried on in Reykjavik) gave its business street a very substantial expression, and two hotels continued

ANSTUR ST., REYKJAVIK.

the agreeable impression that Reykjavik was becoming popular. Photographers are kept busy flattering the vanity of its handsome sons and fair daughters; book-stores supply you with literature of all ages, from "Uncle Tom's Cabin" to the last verses of Thorsteinson; a public library of seventy thousand volumes (in which the *Bulletin of the American Museum of Natural History* may be found) will furnish the visitor with undreamed-of learning, and a cathedral with an organ, a bishop and a choir will save his feet from erring on Sunday; while his incredulous eyes will be shown a public school, a Latin school, a ladies' seminary and a literary club. The last touches of modernity are given in a theater and a jail. Surely those long winter nights, which scarcely leave any day at all, must approach, in the autumn months, shorn of some of their worst terrors. And then there is the coffee house, where coffee, only excelled in Arabia, can be obtained, and languidly sipped to the accompaniment of popular songs on the piano, or in the companionship of garrulous friends. And there is the chess club, which meets on Athalstraeti!

There are two museums in Reykjavik: one a museum of natural history (open one hour a week) and a museum of antiquities—the

THE OXAIA FOSS, THINGVALLIR.

latter over the bank. Both contain admirable specimens and both, it is projected, will be housed with the library in a new public building, where room will be provided ample enough to make these three "foundations" an ornament to the city.

The museum of antiquities has unquestionable importance. Here are very old altar pieces (Christianity was introduced into Iceland in 1000), old vestments and church paintings, with strange archaic buckles, girdles of brass, silver and gold, rugs, carved boxes, old cabinets, swords (many of them strips of iron rust), poignards, stone pestles and mortars, saddles, bits and bridles, lamps and chairs.

The crowning group of objects is a collection of most curious hand mangles, or rollers, for linen fabrics. These "rullur" are made of wood and most elaborately carved, having one uniform form but differing in size and in ornamentation. Some, two and a half feet long, are covered from end to end with carvings, not grotesque, but simple, and rudely or quaintly symbolic and decorative. Glyptic skill has been characteristic in Iceland. I saw some excellent modern examples in snuff horns made from ivory, with carved *motifs* taken from the Icelandic mythology. It seems probable that this ability prevailed more

ALMANNAJA WALL AND ROAD.

in the past than to-day, and may have developed as a recreative feature in the long periods of isolation and idleness. Examples of this old art are difficult to get, and high prices are paid for authentic specimens.

I obtained an antique lamp, in hammered brass from Olafur Sveinsson, the goldsmith and jeweler (5 Austur St., Reykjavik), who deals in every variety of Icelandic curiosities, including belts, brooches, head-dresses, mantles, snuff boxes, bed boards, buttons, bracelets, drinking horns, etc. I paid about four dollars for my little fish-oil lamp and prize it greatly.

From Reykjavik the excursions into the interior are most usually made, though, as I described in a former number, they may begin from the east or northern ports. But the guides and ponies come from Reykjavik and are sent overland. The preparations for a long sojourn in the interior are formidable, especially when the trip contemplated is beyond the zone of habitation and brings the traveller into the tenantless tracts of the middle island. I had no such ambitions or expensive schemes to consider. The ponies represent the vehicle of transport, and none to the accomplished rider could be more acceptable. Their endurance is phenomenal. Two are allotted to each rider, in order to

ALMANNAJA, ASCENDING ROAD BETWEEN WALLS.

change animals. Halts are frequent, where the ponies are considerately treated, and where pasturage is attractive. The ponies feed a little, are remounted, and the journey is continued. Pack ponies carry provisions, clothing and outfit. The guides are unusually intelligent men, many of them teachers during the winter, and are resolute, capable, resourceful and *safe*. They speak English and can thread the devious trails with certainty. In many instances local guides are necessary, as in the crossing of the more difficult rivers.

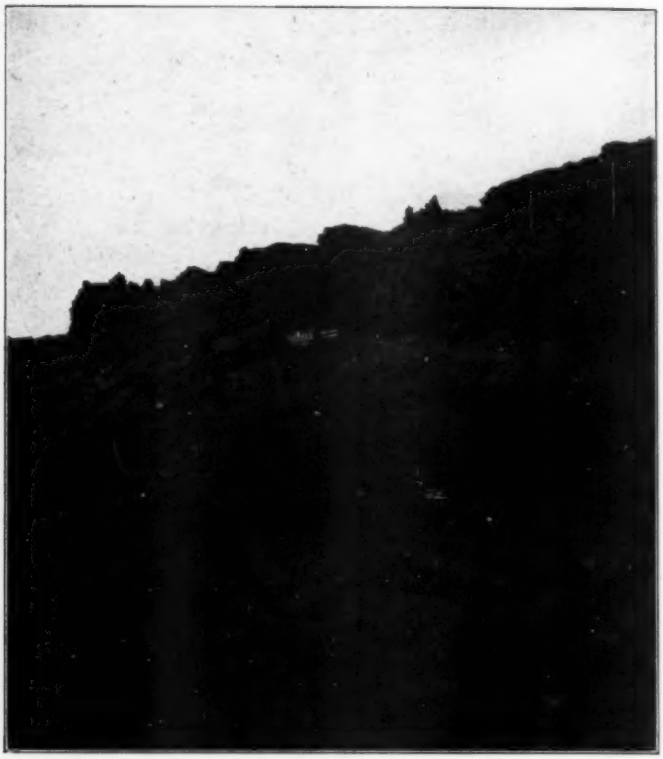
At last my arrangements were completed, and, with some hastily and not very discerningly purchased "canned goods" (they were English and Danish preparations), and some oil-skin clothes and a pair of loaned water-tight boots, my small cavalcade of fine ponies departed for Thingvallir, up Austur street, bound for the distant Gullfoss. As a most unpractised horseman, I had felt apprehensive about my appearance on one of these jogging ponies, and from the ill-concealed mirth amongst the old women on their way to the public laundry on the outskirts of the town, my worst suspicions were justified. On my return to Reykjavik eight days later, I feel no compunctions in stating, I was unnoticed, an excellent testimonial to my improved horseman-

CAIRNS, TO MARK PATHS IN THE WINTER SNOWS.

ship. The easy and instantaneous control over these active animals by the Icelander is admirable. They are all excellent riders, and with bare back or saddle and stirrups shoot over rock-strewn fields with confidence. These ponies are most gregarious and mine would whinny dismally when left far behind by my precipitate companion.

The road to Thingvallir from Reykjavik is excellent, and in places is receiving reinforcement by stone blocks and gutters. It runs for twenty-eight miles (seven Danish miles) and can be used by bicycles and vehicles. Traveling in Iceland is slowly undergoing helpful transformations; the discomforts and, in a measure, the perils diminish with the introduction of roads and bridges though this need not discourage any one who is looking for adventure. The *jökulls* will certainly repel coercion, and many of the rivers at their periods of transporting rage throw off the yoke of bridges. Let the young men, who wish adventure and exposure, suffer from no qualms of disappointment over the disappearance of either from Iceland.

The region first encountered was a hummocky moorland with stony tracts and distant encircling mountain ranges. It grew rapidly more wild and interesting. We reviewed a rolling country with distant hills, near-by vales and valleys, and breezy brows of rising land—an austere,

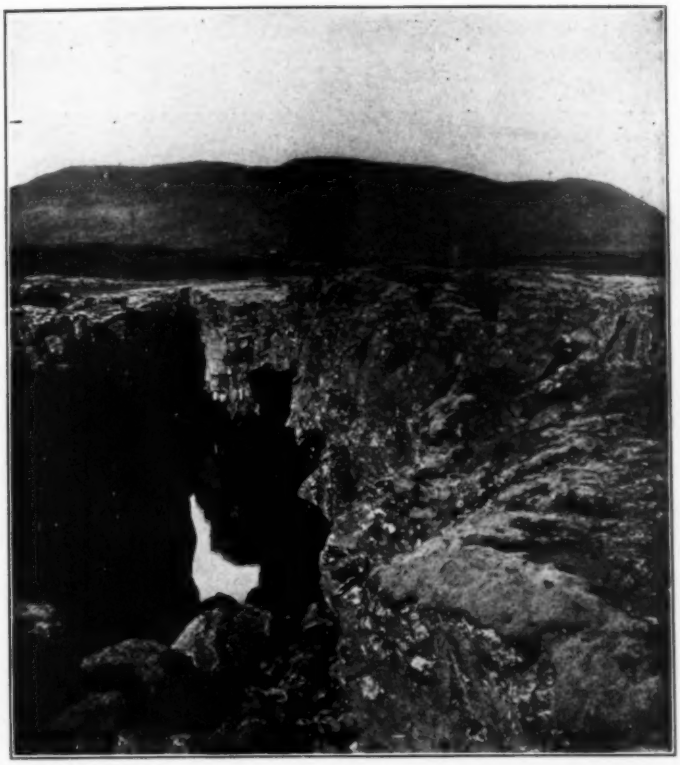


THE ALMANNAJA.

lonely country, full of light, and swept over by cold winds. Then out again we galloped over more spacious areas with intermediate black scoriaceous hills, and here and there in green valleys a farm house. There were lakes and morassy heavily-bedded depressions about us with stony sheets of rubble and wind-swept acres of upland, in which we saw grouse and plover, the latter in numbers. An occasional raven croaked ominously, or protesting curlews whistled at our feet. There were many verdant spots and many more barren ones with the distant snow-covered ranges always in sight. The Thingvallir plain is a remarkably undulating or rather abruptly hilly amphitheater with a rising and falling road.

At last, after a passage across a breezy divide, we came in sight of the great *vatn* or lake of Thingvallir. From this point on the journey gained more and more in interest, and crossing dried-up or running stream-beds, and under high banks, with the mountains, beyond the lake, looming up with peaked summits and snow-gullies, with the occasional appearance of a green oasis about some farmhouse, we drew nearer and nearer to our destination, I with great relief, by reason of a badly bruised and suffering body.

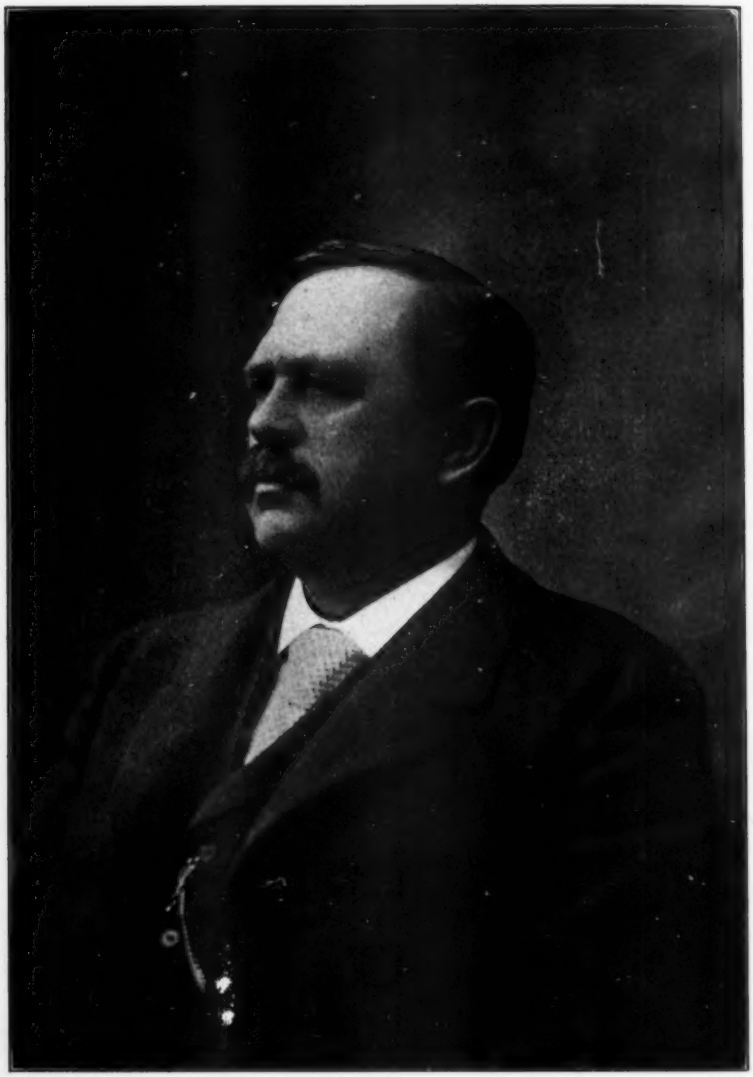
The little red-roofed church, distinguished far off amongst its



CHASM AT THE LOGBERG.

gray and green fields, was seen close at hand, the road began a descent, and, in an instant, the portentous gateway of the Almannaja, like an Egyptian façade loomed gloomily in our path. We moved slowly—awed into temporary silence—down the gradually sloping road between the frowning walls, over a bridge spanning a brawling torrent by a clear, deep pool, and before us, on a ragged plain, which held a fortuitous sort of herbage, fighting its way against the discouragements of a stony soil, was the Walhalla Hotel. To me, at least, it assumed all the radiance of that mythical paradise.

The next day was brilliantly clear, and we studied our locality. It presented a wonderful geological phenomenon. It was a broad valley of depression, between mountains, rifted by long parallel chasms, which crossed it in the direction of its longer diameter, and which were easily descried from a considerable distance, by the furrows they presented in the landscape, by reason of the unequal elevation of their bounding walls. There were some eight of these remarkable fissures—the sundered seams in one vast flooring of erupted rock—and many of them, as that one in which the ancient Logberg stood, contained softly flowing streams of water.



WILBUR OTIS ATWATER,

Late Professor of Chemistry in Wesleyan University, who attained eminence for his investigations in metamerism and with the respiration calorimeter.

THE PROGRESS OF SCIENCE

THE RISE IN PRICES AND THE SALARIES OF SCIENTIFIC MEN

THE extraordinary rise in prices which has occurred in the course of the past ten years—amounting to about fifty per cent. according to the index numbers of the *Economist*—is a serious matter for those who are dependent on fixed salaries, as is the case with most scientific men. It is also an obstacle in the way of the advance of science. Those who should be engaged in scientific research may be compelled to give part of their time to securing the incomes that are needed; some may be diverted altogether from the scientific career, while others may hesitate to enter it. There has always been a kind of panmixia among scientific workers, a lack of severe selection of the most fit. The number of those in this country who have undertaken scientific work does not considerably exceed five thousand, and those who do not prove competent to do work of value are likely to retain their positions in institutions of learning or in the government service. Should there be a negative natural selection drawing the ablest men away from a scientific career, it would be a serious matter, the future of our civilization depending largely on the comparatively small group of scientific men.

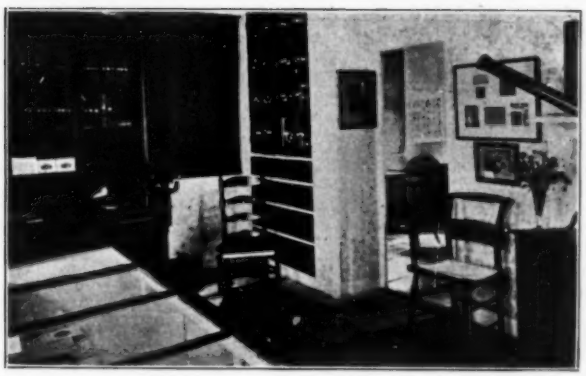
It is a curious fact that it is largely scientific discovery that has lessened the incomes of scientific men. Prices depend on all sorts of conditions, psychological as well as material, but in the end they are determined by the value of gold and the value of gold depends on the cost of production. The cyanide process and other advances in metallurgy, mining and geology, as well as the discovery of new fields, have greatly lessened the cost of producing gold. The world's production of gold in 1896 amounted to 202 million dollars, in 1906 to 400 million dollars, or almost double. Unlike the wheat crop, the annual output of gold is not consumed, and the supply is probably increasing more rapidly than industry and commerce, while at the same time relatively greater use is made of government notes and bank checks. The decreased cost of producing gold tends to make all prices higher, and wages and debts are payable in value less than had been agreed. If the cost of production and the demand for gold should remain constant, there would be an adjustment of the supply; and prices and wages would remain constant on a higher level. But wages reach this level more slowly than most prices, and scientific men and others with fixed salaries suffer. As a matter of fact, both the production of gold and the demand for it will remain subject to great fluctuation, and it seems unfortunate that we can not adopt a more constant standard of value, such as would be obtained by averaging together all staple commodities produced in a series of years, and letting the government issue paper currency payable in these commodities and secured by the property of the nation.

THE MARIA MITCHELL MEMORIAL

MARIA MITCHELL, professor of astronomy at Vassar College from 1865 to 1888, a leader in her science, in the higher education of women and in the movement extending the independence of women, was born in Nantucket in 1818, and was buried there in 1889. The Nantucket Maria Mitchell Association, organized in 1902 purchased at that time Miss Mitchell's birth-

MARIA MITCHELL.

THE MARIA MITCHELL NANTUCKET MEMORIAL.



INTERIOR OF THE MEMORIAL.

place. The building has been fitted up as a center of scientific interest for the community, classes in astronomy being there conducted under the general direction of Miss Cannon, of the Harvard Observatory. This summer Professor Mary W. Whitney, a student of Maria Mitchell and her successor at the Vassar Observatory, spent a week at Nantucket, where she gave lectures and informal talks on Maria Mitchell and recent work in astronomy. It is intended to use the building for natural history as well as astronomy.

In March of the present year a five-inch equatorial telescope, made by Alvan Clark and formerly owned by Miss Mitchell, was given to the association, and it is proposed to build an observatory that will properly house the telescope in a fire-proof building. Efforts to complete this building, to enlarge the equipment and to maintain the work are being made, and those who are interested in the work of Maria Mitchell or in a scientific institution such as is planned for Nantucket are invited to join the association, which they can do by paying one dollar annually or ten dollars as a life member.

SCIENTIFIC ITEMS

WE record with regret the death of Professor Lucien M. Underwood, head of the department of botany of Columbia University; of Dr. Edward Gardiner, of the Marine Biological Laboratory; of M. Maurice Loewy, director of the Paris Observatory, and of Mr. Howard Saunders, the British ornithologist.

A MEMORIAL meeting in honor of the late James Carroll was held by the Johns Hopkins Hospital Historical Club on October 14. Addresses were delivered by Drs. William H. Welch, Howard A. Kelly and William S. Thayer.—The Geographical Society of Philadelphia will hold a meeting on November 6, in memory of the late Angelo Heilprin, founder of the society.—Friends of the late Walter Frank Raphael Weldon, formerly Linacre professor of comparative anatomy at Oxford, have offered the university a sum of about £1,000 for the foundation of a prize, with a view to perpetuate the memory of Professor Weldon and to encourage biometric science.

THE Royal Society has this year awarded its Davy medal to Dr. E. W. Morley, emeritus professor of chemistry, Western Reserve University, and its Copley medal to Dr. A. A. Michelson, professor of physics, the University of Chicago.—Dr. Richard Wettstein, Ritter von Westerheim, professor of systematic botany at Vienna, has been elected president of the Association of German Men of Science and Physicians for the meeting to be held next year at Cologne.

THE American Association for the Advancement of Science meets at the University of Chicago during convocation week, which this year begins on December 30. Together with the American Association meet the Society of American Naturalists and the special societies devoted to anthropology, botany, chemistry, mathematics, physiology, anatomy, psychology, geography and entomology. It is to be hoped that all who are able will plan to attend this meeting—not only professional men of science, but also readers of this journal who are interested in the progress of science. At the New York meeting last year, there were about 2,000 scientific men in attendance, and there is every reason to believe that the Chicago meeting will be equally important.

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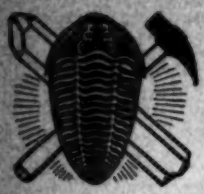

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